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CONTENTS.

PAGE

Familiar British Wild Flowers and their Allies. III.—The Umbelliferæ. By R. LLOYD PRAEGER, B.A. (*Illustrated*) ... 97
The Palæontological Case for Evolution. By R. LYDEKKER ... 100
St. Sophia, Constantinople.—IV. By E. M. ANTONIADI, F.R.A.S. (*Illustrated*) ... 102
Modern Cosmogonies.—II. Criticisms of the Nebular Hypothesis. By AGNES M. CLERKE ... 104
Letters:
"MAN'S PLACE IN THE UNIVERSE."
By ALFRED R. WALLACE ... 107
By AGNES M. CLERKE ... 108
By J. E. GORE ... 109
By W. H. S. MONCK ... 110
British Ornithological Notes. Conducted by HARRY F. WITHERBY, F.Z.S., M.B.O.U. ... 110
Notes ... 110
Notices of Books ... 112
BOOKS RECEIVED ... 114
Clouds as seen from Cloud Level. By Rev. JOHN M. BACON, F.R.A.S. (*Illustrated*) ... 114
Microscopy. Conducted by M. I. CROSS (*Illustrated*) ... 116
Notes on Comets and Meteors. By W. F. DENNING, F.R.A.S. ... 118
The Face of the Sky for May. By W. SHACKLETON, F.R.A.S. ... 118
Chess Column. By C. D. LOCOCK, B.A. ... 119

FAMILIAR BRITISH WILD FLOWERS AND THEIR ALLIES.

By R. LLOYD PRAEGER, B.A.

III.—THE UMBELLIFERÆ.*

AMONG the larger wild flowers which one meets on a country ramble, none are more familiar than the members of the group which botanists call the *Umbelliferæ*, or umbel-bearing plants. These plants are puzzling to the beginner on account of the strong family likeness which runs through the majority. In a large number of the commoner species we find the same strong erect growth, hollow, branched stem, much-divided leaves triangular in outline, and flattish white or pink compound umbels formed of numerous small flowers. Indeed, were stem and leaf the only means of discrimination, identification would be difficult; and if we had to rely on the flowers alone, well nigh impossible. But the fruit of these plants, remarkable in structure, is also much varied in form. Here we have the key to their classification; and a study of the fruits of the *Umbelliferæ* will not only enable the beginner to name his plants, but will present to his view a large series of interesting and beautiful forms. Each fruit consists of two carpels (*mericarps*), often flattened, adhering by their face (*commissure*) to a common axis (the *columella* or *carpophore*), from which they ultimately separate and become pendulous. Each carpel has usually five longitudinal ribs, and often four lesser ribs alternating with these; and in the substance of the wall of the fruit, either under the ribs or in the spaces between them, there are often canals (*vittæ*) filled with essential oil. The mericarps vary in shape, in the character of the ribs, and in the arrangement of the vittæ, and these variations generally render identification easy. If the fruit be cut across horizontally with a knife, its characters are seen to the best advantage. The accompanying sketch shows enlarged sections of the fruits of five familiar Umbellifers, and exemplifies the variety of form and arrangement that has been referred to.

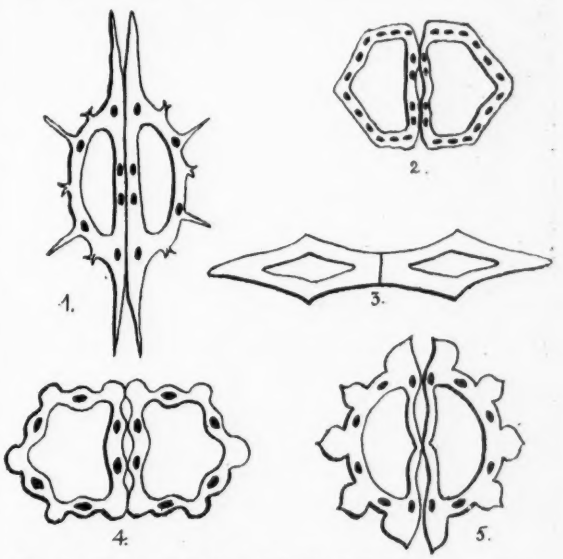

FIG. 1.—Horizontal Sections of Fruits of *Umbelliferæ*. 1. *Daucus Carota*. 2. *Pimpinella magna*. 3. *Hydrocotyle vulgaris*. 4. *Carum Carui*. 5. *Æthusa Cynapium*. Enlarged.

The *Umbelliferæ* are a large order of plants, comprising several thousand species. They are represented in every part of the world, but are especially abundant in Europe and temperate Asia. The species which are reckoned as native or naturalized in the British Islands number sixty-nine, distributed among forty genera; from which figures it may be gathered that a quite unusual number of genera —thirty, to be precise—are with us represented by a single species only. Our British *Umbelliferæ* are all herbaceous plants, but in other parts of the world shrubby species occur, and others again which widely differ in general appearance from the Carrot- or Parsnep-like forms which we are inclined to regard as typical of the order.

Among the British *Umbelliferæ*, some few have inflorescences differing from the characteristic form, the flowers being arranged in simple instead of compound umbels, or in head-like groups. These plants belong to the genera *Hydrocotyle* (Marsh Pennywort), *Sanicula* (Wood Sanicle), *Astrantia*, and *Eryngium* (Sea-Holly); and it will be seen, when we come to speak of the

* For the use of the blocks which form Figures 2 to 6, the writer is indebted to Messrs. C. Griffin & Co. They are taken from his "Open-Air Studies in Botany."

characters of the British representatives of the order, that these genera depart from the usual plant-form of the *Umbelliferæ* in other respects also.

As a group, the *Umbelliferæ* are remarkable for the powerful secretions produced by many species. They are a strong-smelling and strong-tasting order, some useful, many highly poisonous. Among the British species, good exemplifications of both characters are found. Conspicuous among the noxious species are the Hemlock (*Conium maculatum*), and the various species of Water-Dropwort (*Œnanthe*). The poison in these plants is narcotico-acrid, producing delirium, palsy, and asphyxia. The Water-Hemlock (*Cicuta virosa*) is equally dangerous, producing effects similar to those of prussic acid—tetanic convulsions, ending fatally. Others of our British species lose their acidity by cultivation or bleaching. Thus we get our

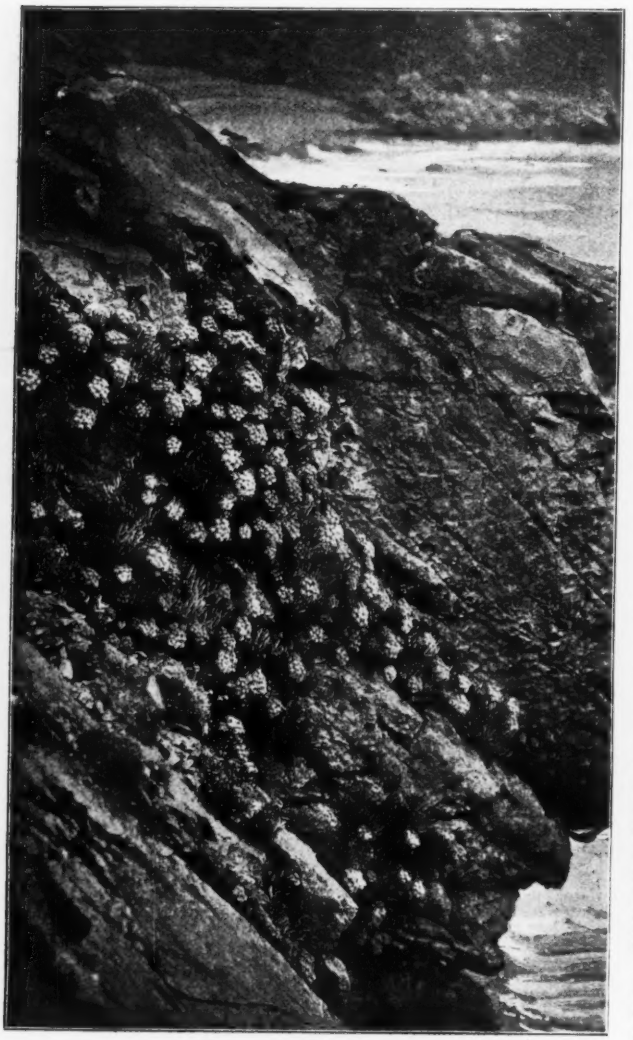

Fig. 2.—The Samphire at Home.

R. Welch, *Photo.*

Carrots and Parsneps, the enlarged tap-roots of species of the genera *Daucus* and *Pastinaca*. The esculent Celery is produced by bleaching the leaf-stems of *Apium graveolens*, a common inhabitant of salt-marshes. The Parsley of our gardens is a curly-leaved form of *Carum Petroselinum*, a plant of unknown origin, which readily escapes and makes itself at home, as a coarse strong-growing plant, on old walls and limestone rocks. Of useful species not in cultivation, the best known is the aromatic Samphire (*Crithmum maritimum*), a plant of the sea-rocks, which makes a delicious pickle. The succulent roots of Sea-Holly (*Eryngium maritimum*) and Angelica (*Archangelica officinalis*) are sometimes candied, and have medicinal properties. The well-known Caraway seeds are the aromatic fruits of *Carum Carui*; and the fruits of Anise (*Pimpinella Anisum*), Coriander (*Coriandrum sativum*), and others, have similar properties.

The British *Umbelliferæ*, nearly seventy in number, form too large a group to permit of a survey by genera and species within the limits of a single article. We may, instead, take up in turn each part of the plant, note its characteristic form or forms, and consider any striking departures from this form which our British species present. First, then, as regards roots and root-stocks. A characteristic form among the *Umbelliferæ* is the tap-root, as we see made fleshy by cultivation in the Carrot. Others, such as the Whorled Caraway and some species of Œnanthe, have a bunch of fleshy fibres, largest near the extremity. *Carum Bulbocastanum*, a rare English species found chiefly near Cherry Hinton, has a brown sub-globular tuber. In the Pig-nut, *Bunium flexuosum*, a common spring woodland plant, the tuberous form is more pronounced, being of irregular shape and comparatively deeply buried; it is sweet and nut-like in flavour, as all of us who have spent our childhood in the country well know. The Sea-Holly possesses far-creeping succulent root-stocks, which burrow deep in the sea-sands, and send up shoots into the light and air above.

The stems of our Umbelliferous plants do not present any great range of form. The upright branched hollow stem of the Wild Chervil (*Chaerophyllum sylvestre*) or Gout-weed (*Ægopodium Podograria*) may be taken as a type. In our larger species, such as the Cow-Parsnep (*Heracleum Sphondylium*) or Wild Angelica (*Angelica sylvestris*), these stout hollow stems, like pillars, designed to bear the weight and stress of the large spreading leaves and umbels, form striking and interesting examples of plant architecture. At each node a solid partition extends across the column, and at the places thus strengthened arise the leaves and axillary branches. In the Marsh Pennywort (*Hydrocotyle vulgaris*) the stem is strikingly different, being weak and creeping, emitting rootlets on one side of each node and leaf and flower-stems on the other. Several other marsh species by their decumbent habit and rooting at the nodes connect this extreme form with the more typical kind of stem. The Samphire, alone among our British species, can produce a perennial elongated woody stem from which annual branches arise.

The leaves of the *Umbelliferæ* are usually characteristic—large, triangular or oblong in outline, and much divided. Sometimes they are long and simply pinnate, as in the Parsnep (*Pastinaca*), Burnet Saxifrage (*Pimpinella*), and Water-Parsnep (*Helosciadium*), but more frequently bipinnate or tripinnate, with finely cut segments; in some cases the final divisions are hair-like, as in the Fennel (*Fœniculum officinale*) and Bald-Money (*Meum Athamanticum*); in other cases, ovate-serrate, as in the familiar Wild Angelica. In the sea-haunting Samphire the linear

segments are glaucous and fleshy, as is characteristic of halophytes, or plants which grow in places charged with salt. In the Whorled Caraway (*Carum verticillatum*) the leaves are pinnate, the pinnæ dividing into numerous hair-like segments which, instead of lying in one plane as in most leaves, spread out all round the leaf stem, like the whorls of the Lady's Bed-straw, making the outline of the leaves cylindrical. The genus *Bupleurum*, or Hare's-ear, includes several slender herbaceous plants, in the British Islands confined to southern England, which are rendered very unlike most of their allies by their simple lanceolate leaves. One species, *B. rotundifolium*, has broad pointed leaves which completely embrace the stem—a unique form among our British *Umbelliferæ*. The aberrant simple-umbelled group which has before been referred to differ from the type in their leaves as well as in their inflorescence. The Marsh Pennywort has peltate leaves like those of the true Pennywort, *Cotelydon Umbilicus*—round in outline with the stalk inserted in the middle. *Sanicula* and *Astrantia* have pretty palmate leaves—a number of segments springing from one point, like a fan. Many of the *Umbelliferæ* are, no doubt, protected from the attacks of feeding animals, large or small, by the virulent or acrid properties residing in their leaves and stems. In one, the Sea-Holly, recourse is had to a device familiar in other families—the production of an armament in the form of spines. In this species, too, which lives on the dry burning sea sands, the leaves are grey and leathery, and protected by thick layers of cells from too great transpiration.

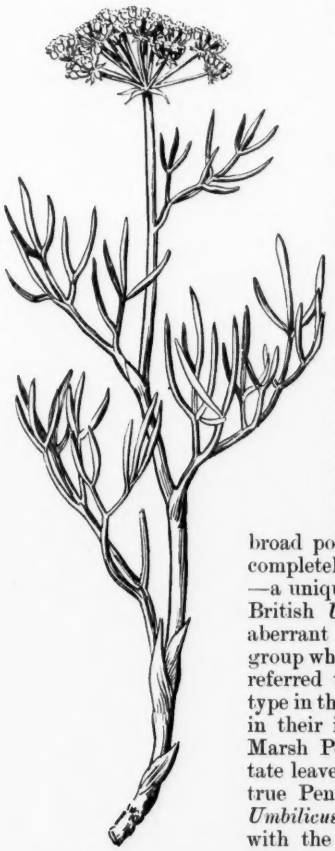

Fig. 3.—Samphire (*Crithmum maritimum*). Half natural size.

Next we come to the flowers, which, as already stated, are usually arranged in compound umbels. That is, the flowering stem divides into a number of branches which spring from one point and reach about the same level; at which level each branch again similarly divides, each division bearing a terminal flower. The result is a large circular flattish surface of blossoms. The advantage to the plant of such an arrangement is that it renders the inflorescence conspicuous—advertises the flowers to the honey-feeding insects, which come and by their visits carry the pollen from plant to plant. In all our *Umbelliferæ*, each flower is quite small; but the result of this aggregation is a most conspicuous inflorescence. Moreover, the outermost flowers of the umbel are often irregular, their outer petals, which alone of all the flowers have room for expansion, being considerably enlarged, thus further assisting to render the flower-mass conspicuous. We may note the same device carried still further in the flat flower-masses of the Guelder-rose, *Viburnum Opulus*, which belongs to an allied order. In this shrub the petals of the outer flowers have been developed at the expense of the essential organs, and these outer blossoms consist merely of a large white corolla—a pure advertisement—while all the business of the plant is carried on by the comparatively inconspicuous flowers which are massed in the centre of the cluster. The flowers of the *Umbelliferæ* are of simple structure. There is little room or need for a calyx, and it is adherent to the ovary, and its free part, which forms the conspicuous protecting sepals in so many flowers, is wanting. The small petals are five in number, wide-spreading, notched, usually of light colour—white, or less commonly pink or yellow. The stamens are five, long and spreading between the petals. The middle of the flower

Fig. 4.—The Sea-Holly at Home.

F. Welch, *Photo.*

is occupied by a disk in the centre of which rise two styles. The honey lies open to all comers. There is no guarding of the approaches to the flower, or to the nectary; no

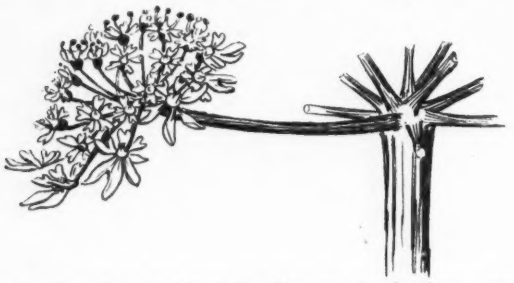

Fig. 5.—Secondary Umbel of Cow-Parsnep, showing large exterior petals. Two-thirds natural size.

provision for the visits of any particular kind of insect. In consequence, all kinds of insects visit the flowers—flies, beetles, bees, moths, ants—both flying and crawling creatures.

The umbels are generally terminal on the branches, but in a few cases axillary, as in the Water-Parsneps (*Helosciadium*) and the Knotted Hedge-Parsley (*Caucalis nodosa*). Compound umbels are found in the majority of the British species, but sometimes the umbels are simple, as in the Venus's Comb (*Scandix Pecten-Veneris*). The number of rays which go to compose the umbel varies greatly, being reduced in some species to two or three, thus forming irregular small clusters of flowers, instead of large disk-like masses. The Marsh Pennywort, already referred to, which has creeping stems from which arise circular peltate leaves, has flower-stems much shorter than the leaves, and quite hidden by them. These peduncles terminate in little clusters of pinkish minute flowers. In *Astrantia*, a quite abnormal effect is produced by the petal-like character of the leaves of the involucre. The involucre is the ring of small leaves which, more or less developed, surrounds the point where the branches of the umbel radiate from the stem. In some plants, as in the Wild Carrot, this involucre is a conspicuous feature, forming a ring of much divided hairy foliage; in a few species it is altogether wanting. In *Astrantia* the umbel

Fig. 6.—Flower-head of Sea-Holly, with Involucre (half natural size), and Single Fruit with Bract (× 2).

is simple, with very numerous rays. The segments of the involucre are longer than the rays of the umbel, and are undivided and erect, surrounding the inflorescence and giving it the appearance of the flower-head of one of the Daisy group of the *Compositæ*. In some of the species, as the pretty *A. carniolica*, the colouring of the involucre adds to the illusion. The Sea-Holly has also an abnormal inflorescence, in which the primary umbel consists of a terminal shoot and several spreading branches arising from one point, and surrounded by an involucre of three leaves, while the secondary umbels which these bear are reduced to egg-shaped heads of flowers, with conspicuous spiny blue involucres, and also a spiny bract below each flower. The Sea-Holly is quite one of the most remarkable as well as one of the most beautiful of our British *Umbelliferæ*. Its leathery, glaucous, spiny foliage, tinged with vivid blue, and heads of blue flowers, are unique features among the numerous native plants of this order.

Of the characteristic fruit of this order something has already been said. It consists of two single-seeded carpels attached by their faces to a common short forked axis; when ripe the faces separate from the axis, the carpels remaining attached and pendulous by their upper extremity. The carpels are sometimes laterally compressed as in Marsh Pennywort (*see* Fig. 1); sometimes dorsally compressed as in the Wild Carrot (*see* Fig. 1); sometimes smooth, often strongly ribbed; sometimes covered with spines. The fruit is usually devoid of any obvious provision to assist its dispersal. When ripe and hanging lightly from the carpophore it is easily detached; the upright stems of the plants are often stiff and elastic, and spring back into position if moved by a passing animal or by the wind. The fruits may often by this means be projected to some distance, in the manner of other catapult-fruits.

The fruits of some, as the Wild Angelica, are broadly winged and may be carried to some distance by a high wind; but in very few cases is there any character which suggests wind-carriage. Of more obvious use are the hooked bristles with which the fruit of some of our *Umbelliferæ* is set. Some of the species of Bur-Parsley, *Caucalis*, for instance, have their fruit thickly beset with hooked bristles, liable to become entangled in the coats of passing animals, and thus secure a wide dispersal. The fruit of the Wood Sanicle is similarly furnished. A curious case is furnished by the Knotted Hedge-Parsley (*Caucalis nodosa*), which bears small almost sessile lateral umbels. The inner fruits of each cluster are smooth, while the outer mericarp of the outer fruits alone is furnished with numerous rows of hooked bristles which are clothed with backward-pointing teeth. The fruit of other species of *Caucalis*, and of the Wild Carrot (*Daucus Carota*) possess an abundance of straight spines which may act in the same way as has been just described. The fruit of the Venus's Comb (*Scandix Pecten-Veneris*) is very curious. The mericarps and carpophore to which they are attached are prolonged into a great beak, which far exceeds in length the fruit itself, attaining sometimes a length of three inches. This beak is furnished with forward-pointing bristles. The low growth of the plant renders it unlikely that its dispersal is assisted by fruits entangled in the fleece of animals, and the object of the remarkable beak is not apparent. In the Sea-Holly, the calyx limb is present—a very rare feature in this order—as five stiff lanceolate segments. These are persistent and crown the rough fruit, and possibly in some measure assist its dispersal (*see* Fig. 6). But though the majority of the *Umbelliferæ* apparently trust to "chance" for their advance into new ground, they still apparently secure a sufficiently wide dispersal, as shown by the abundance of many of the annual and biennial species.

THE PALÆONTOLOGICAL CASE FOR EVOLUTION.

By R. Lydekker.

(*Continued from page* 76.)

It will be well, before proceeding further, to sum up very briefly the palæontological case for evolution so far as the five main branches (or phyla, as Professor Osborn would prefer to call them) of the vertebrate stock are concerned.

With regard to mammals the evidence is practically decisive and complete as to an intimate connection with (and therefore their probable descent from) reptiles. And the same is true with regard to the affinity between reptiles and salamanders. So far as a direct connecting chain is concerned, salamanders indeed cannot be affiliated to fishes, but the collateral, or what may be termed circumstantial, evidence points most strongly to the former existence of such connecting links, and consequently to the evolution of the former from the latter group. Very much the same may be said concerning the resemblances between birds and reptiles.

Although the gaps in the fish-salamander series are still very large, and we are unable to show at present any intermediate form between reptiles and birds, it is important to notice that in not one single instance is there a scrap of evidence which could be construed as detrimental to the evolution doctrine. Such evidence as there is, be it full or be it sparse, in every instance points, so far as it goes, to the derivation of the newer from the older, of the higher from the lower, of the specialised from the generalised.

To revert to mammals, it was mentioned in the course of our remarks on the relationship of that group to the anomodont reptiles that some at least of the small mammalian jaws from the Stonesfield slate and other Jurassic formations, as well as those from the Cretaceous, not improbably indicate the ancestors of mammals other than the modern monotremes. It has been very generally considered that many or all of these Mesozoic mammals were marsupials, but recent changes in our conception of the nature and origin of the latter group have tended to discredit this idea. And the probability is that none of the Mesozoic mammals were marsupials, or at all events not marsupials as we now know them. Far more likely is it that these early mammals—the presumed direct descendants of the anomodonts—laid eggs and approximated in the characters of their skeletons to the modern monotremes. Together with the direct ancestors of the latter (which, as already mentioned, there is some reason to believe may have had a peculiar type of dentition inherited from a special branch of anomodonts) they not improbably formed a primitive group for which Huxley's name of Prototheria is available.

All this is, however, more or less vague conjecture, and it is time to return to facts. Apart, then, from these small Mesozoic forms of which the affinities are unfortunately so uncertain, the most primitive, and, at the same time, some of the earliest (if not actually the earliest) Tertiary mammals with which we are acquainted are the so-called Creodontia, or primitive Carnivora. These creodonts, which varied in size from that of a small fox to that of a bear, were long-jawed mammals, with a dentition of a carnivorous type, but lacking the differentiation of a pair of teeth in each jaw into special cutting instruments—the carnassial, or flesh, teeth of cats and dogs and most other modern land Carnivora. That the creodonts were the direct parents of the latter is now generally admitted by palæontologists,* and it is likewise highly probable that they also gave rise to the Insectivora (shrews, hedgehogs, tenrecs, etc.).

That they were not marsupials (that is to say in the ordinary restricted acceptation of that term) may be regarded as well established; but certain more or less nearly allied forms from the Tertiary deposits of South America appear to indicate such a complete transition from the typical creodonts to the carnivorous marsupials as to render it seemingly very difficult, if not indeed impossible, to draw any satisfactory distinction between the two groups.

* *See* J. L. Wortman, *American Journal of Science*, 1901-2.

And here a few words may be devoted to certain peculiarities in the organization of modern marsupials. In common with the great majority of mammals other than marsupials, the creodonts develop two series of teeth—a small milk, or baby, series, and a larger permanent series, the anterior members of which vertically replace the former as they are shed. Marsupials, on the other hand, if they change any teeth at all, change only a single pair in each jaw, and considerations into which we need not now enter render it probable that their restricted tooth-change is what naturalists call a specialised, and not a primitive feature. In other words, it appears that marsupials have lost the complete tooth-change characteristic of most other mammals, and not that they have only commenced to develop the same. Similarly, recent investigations tend to show that marsupials, like the higher mammals, formerly produced their young with the aid of the structure known as the placenta, of which some of them still retain a vestige. Further, modern marsupials are characterized by the presence of unossified spaces in the bony palate of the skull, and likewise by a peculiar bending-in of the hinder part of the inferior border of the lower jaw—the inflection of the angle of the lower jaw, as it is technically called.

Now the extinct South American mammals referred to above differ from the typical creodonts in replacing a smaller number of teeth, showing in this respect a complete transition from the former to the true marsupials, in which, as already mentioned, only a single pair in each jaw is thus replaced. They show, moreover, the marsupial inflection of the lower jaw, although they lack, in most cases at any rate, vacuities in the palate. As to the presence or absence of a placenta, nothing can of course be said, as indeed is the case with the typical creodonts.

So far as the available evidence goes, these South American sparassodonts, as they are called, seem to justify the statement that between the creodonts on the one hand and the carnivorous marsupials on the other there is such a close connection that (here and elsewhere on the assumption that evolution is the true explanation of the resemblances of animals to one another) there seems every reason for regarding the one group as descended from the other, or both as divergent branches from a common ancestor.

This being so, the question narrows itself as to whether creodonts are more primitive than marsupials. *Primâ facie*, the inflection of the lower jaw and the presence of vacuities in the palate, might apparently be just as well acquired as primitive features; but since the former feature occurs in some of the Jurassic mammals it would seem to be primitive. Both characters may, however, have been lost in the more typical creodonts with which we are acquainted. On the other hand, if, as is generally believed, the reduced tooth-replacement in marsupials is due to degeneration it is manifest that in no sense can the latter group be ancestral to creodonts. And the same seems to be demonstrated by the vestigial marsupial placenta, even if creodonts were non-placentals. The balance of evidence thus seems to be in favour of the derivation of marsupials from creodonts.* It should be added, however, that this is not the view of Dr. Wortman, who has made a special study of the latter group. In his opinion marsupials are not the descendants of placentals, but, together with creodonts, are the derivatives of a non-placental stock. This leaves unexplained the origin of the marsupial vestigial placenta.

* It has been stated that one of the Jurassic mammals had a reduced tooth-replacement like that of modern marsupials, but the minute size of the specimen renders the evidence indecisive.

XUM

Be the exact relationship between the creodonts and the marsupial carnivores what it may, the evidence fully justifies the belief that the two have a common ancestry, and that the latter have given rise to the herbivorous members of the same great group. The most generalised marsupials now living appear to be the American opossums, which we know from fossil evidence to be an ancient family; and it is believed by some that the arboreal type of foot possessed by these animals was a feature of the ancestral marsupial stock. It follows from this (if it be well founded), that if marsupials are derived from creodonts, the ancestral members of the latter group must likewise have possessed feet of the arboreal type, but there is no evidence that such was the case.

It has thus been shown that, apart from details, palæontology enables us to refer such widely divergent groups as the modern marsupials and carnivora, as well probably as the insectivora, to a common ancestry; the ancestral type being not improbably represented by the early Tertiary creodonts, or by nearly allied forms which may have existed at a still earlier epoch.

But the importance of the creodonts does not by any means end with their resemblances to the true carnivora on the one hand and to the carnivorous marsupials on the other. They are equally nearly related to the early Tertiary ancestors of the modern ungulata or hoofed mammals, the so-called condylarthra of the American palæontologists. And, indeed, the great difficulty connected with the study of the early Tertiary mammals of all kinds is not to discover relationships, but to point out differences; all the modern ordinal groups appearing to merge more or less completely into one another at that comparatively early epoch of mammalian history. To describe the generalised character and mutual resemblances of these early Tertiary mammals in an article like the present would, however, be difficult as well as wearisome, and it must suffice to record the important fact that the further we recede in time from the present epoch, the less differentiated become the different groups of mammals.

(To be continued.)

ST. SOPHIA, CONSTANTINOPLE.

By E. M. Antoniadi, F.R.A.S.

Illustrated from original drawings by the Author.

IV.

We have seen that a representation of the sky itself was deemed the only vaulting worthy of Justinian's church; and it now remains to show that it was not really possible to imitate the firmament, with brick and mortar, better than we find it realized in St. Sophia. A flattened dome, resting on emptiness, is the general impression which the sky leaves to mankind. But the impossibility of a close architectural reproduction of that impression necessitated an appeal to indirect methods; and thus was it that a recession of the supports became the only available structural equivalent of empty space.

In this connection, it is important to remember that any increase in number of the supporting arches entailed a corresponding diminution in the projection of the pendentives; and that the concomitant effect tended to defeat, in some measure, the very idea of the dome, first by exaggerating the visibility of its supports, and then by causing it to rest directly on earth, through an uncouth cylindrical drum or tower. Considerations like these must have been always present in the mind of Anthemius; and his solution of the problem was stamped with that direct simplicity so characteristic of the master mind. He obtained the "flattened" appearance by limiting to 108°* the arc subtended by the cupola, whose height thus equalled only one-fourth of its diameter; and he secured a maximum of aeriality to the supports by increasing the projection of the triangular pendentives to a maximum, and by limiting to a minimum of four the number of supporting arches. The gentle, but forcible, lightness of the effect produced by those lofty walls, so boldly projecting into space, is further enhanced by the vast expanse of the semi-domes to east and west and the arches to north and south, undermining, so to say, on all points the effective supports of the hemisphere. Resting thus everywhere on sharply receding surfaces, and further alleviated by forty windows opened at its base, the lofty dome of St. Sophia has well remained to this day the most successful architectural approach to the air-borne vault of the universe.†

A restoration of the eastern bay of St. Sophia, prior to 1204, is given in Plate II. The view point is the same as in Plate I., but the angle embraced is much smaller; and, in order to show as clearly as possible the details of the choir, the confusing ocean of lamps has been to a large extent eliminated.

In the centre we find the Ambo, from which the Gospel was read, and where the Greek emperors were crowned.‡ Much uncertainty is veiling the form of the pulpit,§ and the representation here given of it does nothing to clear matters. The Ambo has been drawn out westwards in

* This value was increased to 160° in the reconstruction.

† It is perhaps unnecessary to revert to the fact that the cross, mentioned by the poet, towered majestically above the dome, as the logical coronation and trophy of the Great Church of Christendom; and that the All-Ruler (Παντοκράτωρ) adorned in mosaic the inner circular crown. The arguments given by the writer on pp. 30, 84 and 91, which lack of space has rendered laconical, have established these truths on a strong basis. Still some additional evidence would not be out of place here. Three hundred years after the second dedication of St. Sophia, Basil the Macedonian raised a large and beautiful church in the palace of Constantinople, and we hear from the patriarch Photius that no cross, but a Pantocrator, was depicted inside the dome of the new church. Considering that St. Sophia was the standard for imitation, and that its example was law in such matters, the fact mentioned by Photius assumes the force of a grave objection to Mr. Burkitt's theory that the cross was inside, and that the dome of St. Sophia, destitute of its outer cross, looked very much like a modern astronomical observatory. But it is possible to bring forward arguments of a much more decisive character against that artificial position. Du Cange, whose extraordinary accuracy caused him to write nothing without proofs, says:—"In interiore tholi. . . centro. . . Iustinianus opere musivo Christum in iride sedentem, orbem judicantis effigie, describi curavit, ut αὐτόπτει testantur" (*S. Sophia*, § 33). What more can we ask, as corroboration of the fact that the Pantocrator was originally in the dome, than this proof of ocular witnesses?

Notwithstanding this converging evidence, perhaps the strongest proof that the cross was outside is afforded by an examination of the Silentiary's poem. If, as suggested by the writer (on the authority of Homer, Il. P, 599–600: "γράψεν δέ οἱ ὀστέον ἄχρις αἰχμὴ Πουλυδάμαντος," "and the lance of Polydamas *cut* him as far as the bone"), ἔγραφε was used by the Silentiary as meaning *incised, carved*, it would then be probable that the same sense may occur more clearly in another part of the poem. Checking our anticipation by the evidence of the hexameter, we find it verified to the letter: thus between verses 274 and 294 we learn that γράφειν is used as a synonym of χαράσσειν, *to incise, to carve, to engrave*, and that χαράσσειν means *to cut right off, to hew*, in verse 246 of the "Descriptio Ambonis." And last, but not least, we find the poet, in his description of the *Opus Sectile* of the gallery (composed, as we know, of inlaid pieces of marble), connecting the verb ἔγραφε with its subject λαοτόρος, *sculptor*, whose office was certainly not to paint, but to bore, and to cut, stones with a chisel.

‡ *Cantacuzenus*, ed. Bonn, pp. 196-202.

§ It is not certain that the Ambo of St. Sophia differed very much from the great pulpit of St. Mark's, Venice, which also has two sets of colonnades. It must have been, however, much larger, as holding many people at the coronation ceremony.

XUM

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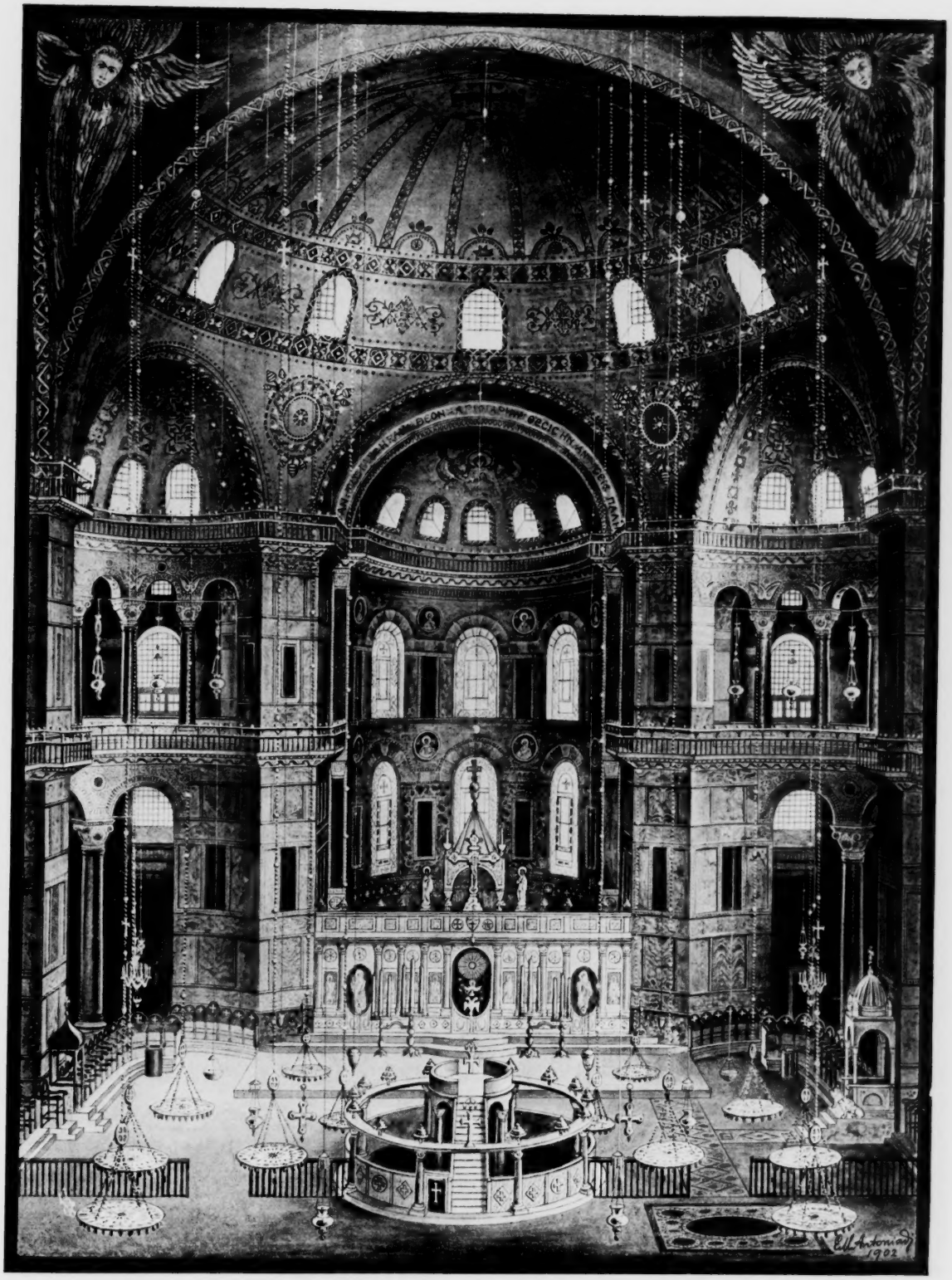

THE EASTERN BAY OF THE GREAT CHURCH, ST. SOPHIA, CONSTANTINOPLE.

Probable View of the Choir and of the Sanctuary in the tenth century.

XUM

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this Plate, so as not to interfere with the view of the Holy Screen beyond. Then we have the enclosure of the Soleas, on each side of which were the Singers' Stalls, headed by the Imperial Throne and its two lions to the right. The stalls of the Choir are shown to continue parallel to the axis, and not curving into the cylindrical walls of the conchs, as in that case the voice of the singers would be inaudible from some points of the nave. Huge candlesticks would stand in front of the Iconostasis, while the Holy Door must have been crowned by the two-headed Greek Eagle.

Lastly, beyond the twelve columns of the Iconostasis, the worshipper would see towering the Ciborium (baldachin) of the Altar, surmounted by a cross resting on a globe, symbol of the expansion of Christianity over the world's surface.*

We learn from the Silentiary that originally the whole extent of the first floor level was reserved to the women. Gradually, however, the increasing numbers, offices, and requirements of the clergy, caused them to invade the galleries, and to permanently establish themselves in the southern and south-eastern divisions of the right aisle. A fine marble screen, still extant, separated the central hall, or *Great Secretum*, from the rest of the Catechumena, while the dome immediately to the east represented the Pentecost in mosaic—a fitting subject to the ecclesiastical councils and to the Synod of 869, held in this part of the church.

The western section of the Gyneceum was known by the name of *Catechumena of the Narthex*, as extending above the vestibule. We know that veils used to stand here, screening the women from the nave below, and the iron ties of the bolts, from which the veils were suspended, have been seen in position by the writer in 1902, both on the piers, as well as on the twin columns over the Royal Gates.

The structure is full of symbolism, and the reasons for the presence of lily, vine leaves, fishes, dolphins,† etc., in the decoration are obvious. So also is the groupment, or division, in three of the windows. Numbers to whom preference was given seem to have been 2, 3, 4, 6, 7, 8, 9, 12, 16, 24, 32, 36, 40, 48, 60, 72, 96, 100, 120, 160, 180, 220, 240, 260, and 300. The mystic number 40 is repeated many times:

(1) In the diameter, in Byzantine feet, of the cylindrical opening of the eastern apse;

(2) In the diameter, in feet, of the four exedras;

(3) In the length, from column to column, of the corner divisions of the aisles and gallery;

(4) In the height of the first cornice;

(5) In the mean height of the vaulting in the aisles and Narthex;

(6) In the total height of the four domes in the angles of the first floor;

(7) In the number of columns of the floor;

(8) In the number of columns of the gallery, towards the nave; and

(9) In the number of ribs, piers and windows of the dome.

According to the writer's own measurements, the side of the square area covered by the pendentives and by the dome is 102 English feet and 2·4 inches, or 31·15 metres. But this represented 100 Byzantine feet. Hence one Byzantine foot is equal to 1·0220 English, or 0·3115 metres.

* The true mosaic decoration of the eastern semi-dome is not known. Plate II. gives its present decoration by Fossati, while the "Mystic Lamb," shown in Plate I., is merely intended to symbolize our agnosticism on the subject.

† The dolphins and trident of Neptune were an old emblem of Byzantium.

The vaulting of the church rests on the piers and outer walls, as well as on 104 columns, but of which only 96 are round. Considering that this quantity is the multiple of so many numbers as 2, 3, 4, 6, 8, 12, 16, 24, 32 and 48—all favourite numbers—its choice was certainly premeditated, and not the result of chance.

Fig. 8 shows a typical mediæval Greek capital with the monogram of Justinian, carved out of the marble, and repeated on the brass ring. Probably, as remembrances of paganism, ancient Greek capitals were rejected.

The great verd-antique shafts, separating the nave from the aisles, are not all equally strong. Four of them have a diameter of $42\frac{1}{2}$ inches, while the other four measure only $38\frac{1}{4}$. The thin columns have been erected in the centre of the sides, the stronger ones near the piers. Still, the first impression is that the colonnades are uniform; the extreme

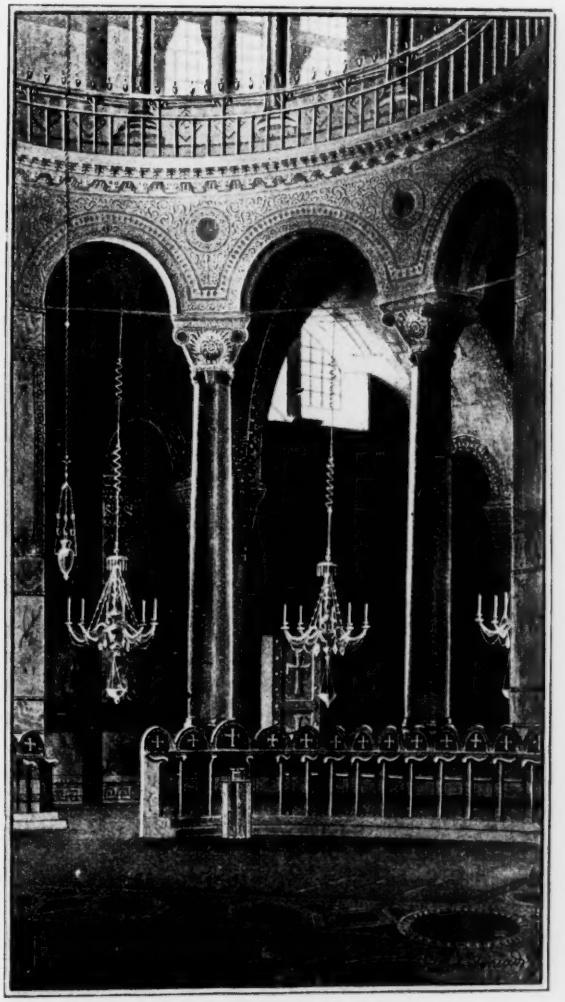

FIG. 9.—A View of the south-eastern Exedra, and of the Great Porphyry Columns, showing the Singers' Stalls and Metatorion in the background, at the time of the Emperor Constantine Porphyrogenitus.

shafts seeming reduced in thickness through the dwarfing vicinity of the piers.

The treatment of the supports in the four exedras (Fig. 9) required much more delicacy, inasmuch as it was deemed desirable to render the importance of the conchs

subservient to that of the great central colonnades under the dome; and yet it would have been dangerous to tamper with the solidity of the shafts, since it is on the exedras that devolves a considerable part of the thrust exerted by the great arches, pendentives and semi-domes, and, indirectly therefore, of the dome itself. A reduction in height and diameter of the exedra columns secured their subordination to the grand colonnades; while the loss of solidity thus incurred was in some measure atoned for by the judicious choice of carmine Theban porphyry—the strongest stone used in the church—as an effective check to the tremendous weight of the vaulting above.*

If we revert to our plan, in the March number of KNOWLEDGE, we shall find that the abutments of the dome are fairly satisfactory on all sides; although ill calculated to remain unbuttressed for many centuries in a soil visited by frequent and violent earthquakes. But the abutments of the lateral walls will be judged at once quite inadequate to the wide spans of the aisles and galleries; a defect further aggravated in the angles by the additional thrust of the exedras. It was, therefore, impossible for Justinian's church to have stood long unbuttressed. Trouble must have very soon made itself felt, first in the angles, then in the midst of the north and south sides as well as to the east, right and left of the Sanctuary, and lastly, after several centuries, in the very abutments of the semi-domes and of the dome.† Historical evidence confirms these deductions to the letter. We find, accordingly, the south porch, and of course the lateral ascents, in position as far back as the reign of Theophilus, 275 years only after the second dedication, whereas the sturdier piers of the nave opposed, unbuttressed, a gallant resistance to the earthquakes of 754 years.

But, in framing these criticisms, we should not be apt to forget that, if the great architect, to whom we owe that marvellous conception, showed himself fallible, in underestimating the effects of thrust, he was necessarily lacking that experience which we acquire to-day by a mere glance thrown on the buildings of his time, now fourteen centuries old. Domed structures in the beginning of the sixth century were comparatively new, few, and unimportant. Nor could the vast, but grovelling, cupola of the Roman Pantheon yield any object lesson in the propping of an aerial dome at the height of 130 feet.

A word or two on the solidity of St. Sophia in the present day, and we will have finished with these disjointed fragments. The part of the church which has suffered most is the south angle, where the column of the gallery has been thrust nearly 7° out of the vertical (Fig. 10). But a strong double buttress, rendered immovable by the weight of the minaret erected by Mohammed II., checks any further inclination of the column. The arches to north and south are smaller by 28 feet than those to east and west, and, having proper support in the plan, are fairly strong. As in the past, danger is to be looked for in the great open arches to east and west. Inasmuch as the four main piers are well buttressed from without, their failing in a direction parallel to the lesser axis is an impossibility. The age of the eastern arch is 540 years, and this part of the vaulting seems to be in excellent state, as the stone masses erected by Andronicus Paleologos consolidate all the east front. But the western arch is 920 years old, and notwithstanding its buttressing, together with the great niche, by an additional thickness, has been deformed by the thrust of the cupola, the result being an exaggerated concavity of the semi-dome. Moreover, this arch has suffered much during the earthquake of 1894, and its last repair has necessitated the removal of the fine mosaic eikon of the Virgin at the crown. And thus it is that the possibility of an eventual rupture of equilibrium along the major axis, to the west, is the chief cloud in the solidity of the edifice. Still, the danger is certainly far from imminent, so that there are excellent reasons to hope that the venerable dome of the Eternal Wisdom, hallowed by the universal verdict of nations, and reverenced by the ravages of time, will continue inciting, for long centuries to come, the pious fondness and genuine admiration of mankind.

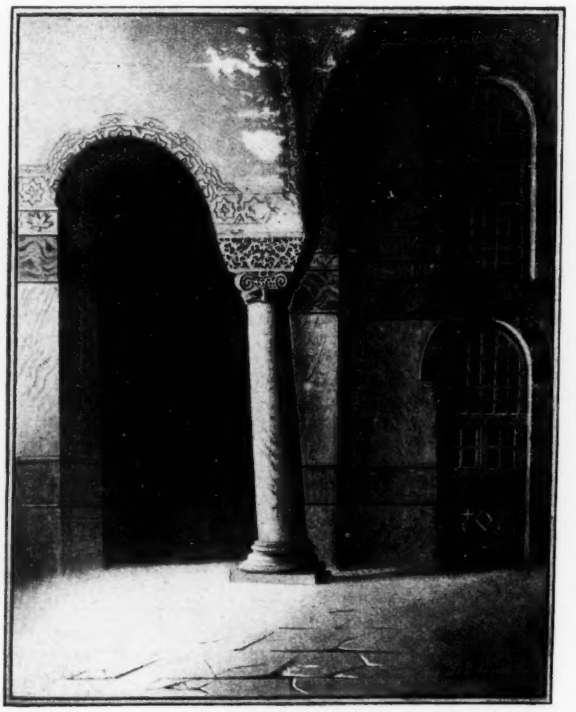

FIG. 10.—Dangerously-inclined Column in the right-hand Catechumena, or first floor level.

* Probably these shafts were not long enough, so that they may have facilitated, or even decided, the subordination in question. In his work on *Aya Sofia, Constantinople*, London, 1852, p. 2, Fossati says that during the reparations conducted by him from 1847 to 1849, he brought back to the vertical thirteen columns of the Gyneceum which had been pushed out by the thrust of the lateral arches. Great credit is, therefore, due to Fossati for this architectural triumph, by which he has so powerfully contributed to the consolidation of the church.

† In his restoration of the exterior for 1300 (Fig. 3) the writer has eliminated part of the buttressing of the aisles, with a view to showing as clearly as possible the original design of the church.

MODERN COSMOGONIES.

By AGNES M. CLERKE.

II.—CRITICISMS OF THE NEBULAR HYPOTHESIS.

LAPLACE'S theory was a perfectly definite conception. In this lay its distinctive merit; in this also its special susceptibility to attack. Here was no question of condensations round nuclei arising at discretion amid the large possibilities of boundless elemental confusion; but of an orderly succession of occurrences, rendered inevitable by the steady operation of mechanical laws, and harmonising, in their outcome, with the array of ascertained phenomena visible in the planetary system. These, accordingly, ceased

to be regarded as arbitrary or casual; they became linked together, in the present, and with the past, as joint products of one grand scheme of development. The mode of origin of the bodies exhibiting them accounted, its inventor claimed to have shown, simply and entirely for them all; and at least the fundamental propositions laid down by him could not be gainsaid. Clearly, the unanimity of planetary movements is no result of chance; it represents, quite obviously, a survival of the general swirl of an inchoate mass, occupying primitively the whole recognised sphere of solar influence. Ambiguities set in only when details come to be considered. The engendering nebula devised by Laplace was provided with a vast endowment of heat, and a slow movement of rotation; hence cooling, contraction, and acceleration advanced *pari passu*, the last as a consequence of the dynamical law by which the algebraic sum of the areas described by any number of bodies round a given axis, multiplied by their several masses and projected upon a single plane, remains constant to the end of time. In other words, the moment of momentum of a congeries of particles can neither increase nor diminish, unless through the action of an external force.

The nebula then quickened its pace until a stage was reached at which centrifugal speed could no longer be controlled by gravity; separation became inevitable, and an equatorial ring was abandoned, which thenceforward revolved on its own account in the period conformed to by the undivided mass at the epoch of its secession. This was the first of many subsequent crises of instability, each eventuating in the detachment of a nebulous ring. These rings, however, were regarded as merely transitional forms. They survived, just for illustrative purposes, in the Saturnian system; elsewhere they broke up into fragments which ultimately coalesced into globes; and the globes were embryo planets. There was indeed a hitch in the line of argument which did not escape the acumen of the French geometer. The direction of the axial movement imparted to the members of the solar family depended essentially upon the relative velocities of the portions of matter brought together for their construction. If the inner sections of the self-shaping mass moved faster than the outer, the resulting rotation should have been retrograde; if slower, direct rotation would have ensued. Now, in a ring like that of Saturn, composed of discrete particles, linear speed decreases continuously outward, each of its minutest constituents obeying independently Kepler's law of periods and distances. Such a formation would therefore have been unfit for the purpose in view, and Laplace accordingly substituted an annulus endowed with a considerable amount of cohesion, and capable of rotating, like a solid, in a single period. It is true that such unanimity of movement was incompatible with the other postulated conditions; but the anomaly escaped notice for above half a century.

The prescribed genetic process, at any rate, whether workable or not, was a strictly regulated one; its steps were marked with characteristic precision. Yet by this very determinateness it gave hostages to the future. It challenged the application of tests which designs more vaguely sketched might have evaded. The primary criterion of its truth was the prevalence of concordant motion throughout the solar domain. The possibility of counter-currents was formally excluded. Hence the discovery of the retrograde systems of Uranus and Neptune flatly contravened its pretensions to unconditional acceptance. With less evidence, but equal certainty, Laplace's hypothesis involves the consequence that each planet circulates in the identical time occupied by the rotation of the undivided nebula just before instability toppled over into separation. Each of the planetary periods should accordingly bear a certain ratio, prescribed by inexorable mechanical law, to the actual period of the sun's rotation. In point of fact, however, the periods in question are much shorter than comports with the necessity for the conservation from age to age of the system's moment of momentum. The discrepancy was adverted to forty-two years ago by M. Babinet.* He showed, in March, 1861, that the axial movement of the solar mass, when distended to fill the sphere of Neptune, should have been, by the law of areas, so excessively slow that more than 27,000 centuries would have been needed for the completion of a single rotation; while the period, even when the shrinking nebula had come to be bounded by the terrestrial orbit, must still have been protracted to 3181 years. Under these circumstances, centrifugal force would never have over-balanced central attraction; no rings could have separated, and no planets could have been formed.

Quite recently, Mr. F. R. Moulton of Chicago† has reconsidered the subject in the course of a careful and candid discussion of the difficulties besetting the Nebular Cosmogony as viewed from the standpoint of modern science, and he comes to essentially the same conclusion. His calculations, though founded on data expressly chosen so as to give the classic theory the benefit of every doubt, made it perfectly clear that the moment of momentum of the embryo planetary system should have exceeded its present value no less than 213 times if, when it extended to the distance of Neptune, it rotated in what is now the period of Neptune. But moment of momentum is a constant. The lapse of millions of years makes no difference to it; it can neither have increased nor diminished since the sky was first flecked with the "breath-stain" that was appointed to condense into our sun, which, at every stage of its subsequent evolution, must, in this respect at least, have maintained immutability. On the other hand, this being so, its primeval wheeling motion would have been much too leisurely to permit the occurrence of accesses of instability. Gravity would have steadily kept its supremacy over the forces tending to disruption until the nebula had contracted to less than the compass of the Mercurian sphere; and its overthrow at that epoch would have been too late for the origination of any of the sister-orbs of the earth. These results, it is true, depend in part upon the mode of variation in density ascribed to the progressively shrinking nebula; but the law adopted by Mr. Moulton has a consensus of authorities in its favour. Nor could its deviation from exactitude—if it be inexact—possibly suffice to account for the enormous discrepancies brought to light by its aid.

The Nebular Hypothesis stipulates further that satellites must revolve more slowly than their primaries rotate. The reason is patent. In the periodic time of a detached body the rate of gyration of the original mass is, if the theory be valid, perpetuated. Subsequent contraction tends to quicken, and very greatly to quicken, the rotation of the planet, while the period of the satellite survives unaltered as a standing record of what the joint period was. This relation may indeed be modified by the effects of tidal friction; but it is more than doubtful whether it can ever be reversed. It is, then, a characteristic feature of the mode of evolution described by Laplace that no month—so to speak—can be shorter than the corresponding day. And the rule is conformed to in nearly every part of the solar system. Nevertheless, two flagrant violations of it have lately obtruded themselves upon notice, and can scarcely be explained away by supplementary hypotheses.

* *Comptes Rendus*, t. LII., p. 481.

† *Astrophysical Journal*, Vol. XI., p. 103.

The first ascertained anomaly of the kind was met with in the swift circulation of Phobos, the inner satellite of Mars, which completes three revolutions, and enters upon a fourth, while the planet attended by it wheels once on its axis. The fact is most perplexing; and the confident persuasion that solar tidal friction would avail to remove the difficulty has not proved well grounded. Solar tidal friction, it may be remarked, acts as an external force upon subordinate systems submitted to its influence. Within their precincts, moment of momentum may be destroyed by it; and it was hence a feasible supposition that the rotation of Mars had, in the course of ages, greatly slackened through the retarding effect of sun-raised tides. But the agency was demonstrably inadequate to the task assigned to it. The reduction of the rotational moment of Mars to about one twenty-fifth its primitive amount* would have brought other consequences in its train, at least one of which did clearly not ensue. At an early stage of the process, Phobos should have been re-engulfed in the parent body.† For the pull of the small tidal wave raised by it on the surface of that body would have been backward from the instant that the balance of periods became inclined, through solar compulsion, in a direction contrary to that it would naturally have taken; and the loss of velocity must have entailed its descent, along a spiral path, towards an inevitable doom. The continued existence, then, of the little satellite closes this way of escape from the difficulty raised by the shortness of its period. M. Wolf had recourse to a different explanatory subterfuge.‡ He believed that Phobos might have owed its origin to one of Roche's "elliptic sheddings" of nebulous matter dropped downward from near the polar regions of the distended Martian spheroid, and rotating, owing to its low rate of linear speed, in the immediate vicinity of the cooling planet. The explanation, though ingenious, is too recondite and evasive of mental grip to be satisfactory.

The Saturnian system exhibits a case of the same kind, but still more perplexing to speculative prepossessions. Saturn's ring system has always appealed to thinkers as a striking object-lesson in nebular development. It forcibly arrested Kant's attention, and he sketched its birth-history on lines anticipatory of those adopted by Laplace for the solar system in its entirety. Laplace himself regarded the formation as the one surviving relic of the annular stage of planet-building—as a witness from the dim past to a condition of things elsewhere transitory. Yet the witness has turned king's evidence, and betrayed the whole situation. The innermost Saturnian ring has a period far too short to be compatible with the requirements of theory. For its meteoric constituents, which are known to revolve as independent units, complete their circuits in between five and six hours, while the planet needs just $10\frac{1}{2}$ for its axial rotation. Moreover, tidal friction is here far less available than on Mars; yet no other retarding agency has been invented. The deadlock appears final and hopeless.

An objection quite as formidable, and even more fundamental, was raised by Kirkwood in 1869. The nebulous material of the uncondensed sun must have been, at the outset, of the last degree of tenuity. Atmospheric air is, by comparison, a dense and massive substance. Yet no reasonable person could ascribe to aerial matter the least power of resisting strain. We know perfectly that a rotating globe of air, and, *à fortiori*, a globe of matter thousands of times less compact than air, would unintermittently disintegrate at the surface with the progress of acceleration. The disturbance and restoration of equilibrium would be virtually simultaneous. There could be no accumulation of internal stress, and consequently no distinctly separated epochs of instability. At the first solicitation, at the first instant that centrifugal velocity gained the upper hand over gravity, nebulous wisps would have become detached; and their detachment would have gone on without pause. Space would have been strewn with the *débris* of the condensing nebula; and there should have resulted a vast cloud of cosmic dust, not a majestic array of revolving spheres.

Moreover, even if the nebulous material had possessed the fabulous cohesion indispensable for its division into voluminous rings with wide intervening empty gaps, their ultimate agglomeration into planetary globes would never have been effectually accomplished. Kirkwood long ago questioned the feasibility of the process; Mr. Moulton has gone far towards demonstrating that it must have had an abortive outcome.

Another grave objection to Laplace's scheme is founded on the marked deviations visible in the solar system from conformity to a fundamental plane of motion. Apart from unexplained modifying influences, all the planets should circulate along the level of the sun's equator, and rotate on axes perpendicular to it. How far this is from being realised in nature we have only to look around us to perceive. We owe the changes of our seasons to the tilted fashion of the earth's spinning. Yet it is by no means easy to understand how the pole of its equator comes to be situated in the Tail of Ursa Minor, while the pole of the ecliptic is involved in the folds of Draco. They should have coincided if the simple prescription of the Nebular Hypothesis had been followed in the making and modelling of the planets. Nor are the terrestrial arrangements exceptional. The Saturnian equator and the Saturnian rings have a still higher inclination; while, in the systems of Uranus and Neptune—since we may thus interpret their retrogade revolutions—the angle exceeds the limit of a quadrant. These and other similar discrepancies prove the solar mechanism to have originated by a more complex method than that imagined by Laplace; and an hypothesis which invokes the aid of a multitude of auxiliary devices for its extrication from accumulating embarrassments falls thereby under the suspicion of not being worth the trouble of extricating. It forfeits, at any rate, all claim to commendation for directness and simplicity.

The Cosmogony turned out at Paris has thus proved vulnerable on a number of points; but all the blows aimed at it have not told with such deadly effect as those just referred to. Some have fallen harmlessly, or glanced aside. One hostile argument in particular, which for a time seemed irresistible, has been completely overthrown by the logic of facts, and deserves mention only as a historical curiosity. Towards the middle of the nineteenth century, the progress of sidereal astronomy seemed to take the direction of showing all nebulæ indiscriminately to be of stellar composition. With Lord Rosse's great reflectors, a good many such objects were genuinely, and some besides were deceptively, resolved into stars, the illusory effects being confirmed by Bond's observations with the deservedly celebrated fifteen-inch refractor then recently built by Merz for Harvard College. Hence the rash inference was drawn that resolution was wholly a question of optical power, and that no real distinction existed between the stellar and the nebular realms. Herschel's "shining fluid" assumed a mythical air; "island-universes" came into popular vogue; and all but a few careful thinkers held nebulæ and clusters to be differentiated merely by degrees of remoteness. But if space contained only full-grown stars, and no stars in the making—no star-spawn, no star-protoplasm—then the imagined

* Moulton, *Astrophysical Journal*, Vol. XI., p. 110.

† Nolan, *Nature*, Vol. XXXIV., p. 287.

‡ *Bull. Astr.*, t. II., p. 223.

evolutionary history of our system was left in the air, destitute of even the most fragile prop of observed fact.

From this precarious position it was rescued, partly by the cogent reasonings of Whewell and Herbert Spencer, finally and triumphantly by Sir William Huggins's spectroscopic discovery of the cosmic gas "nebulium." Since August, 1864, there has been no possibility of denying that the heavens contain ample stores of just the kind of material Laplace wanted, though whether it played just the part he assigned to it, in the manner that he supposed, is a question to be answered with profound and growing reserve.

An objection of late urged against the Nebular Theory, from the standpoint of the kinetic doctrine of gaseous constitution, is of much speculative interest. From a gaseous nebula equal in mass to the sun and planets, and distended sufficiently to fill the orbit of Neptune, there should have been, if the prevalent opinion be correct, a rapid leakage into space of its lighter ingredients. Of hydrogen and helium, we are told, it should infallibly have become depleted; yet there is no lack of either in the sun of the twentieth century. Their retention, it must be admitted, is, on the hypothetical conditions, difficult to account for. The "critical velocity" at the limiting surface of the supposed nebula would have been 4·8 miles a second. This is, in fact, at the distance of Neptune, parabolic speed. The planet itself, if it could attain to it, would break the bonds that bind it to the sun, and seek its fortunes under some different allegiance. Similarly, any particle of the primitive nebula thus accelerated should have become an irreclaimable vagrant. Now the velocity of hydrogen-molecules at the zero of Centigrade is, in the mean, about $1\frac{1}{6}$ miles a second, but attains, in the extreme, to above seven miles. Hydrogen could not then have been permanently retained by the solar nebula; and the escape of helium would have more slowly ensued. Yet these results, though seemingly inevitable, did not, in actual fact, come to pass; either because the generating body was differently constituted from what has been supposed, or because countervailing influences were brought to bear. It is, for instance, amply possible that the dynamical condition of gases may be essentially modified by rarefaction carried to a degree transcending the range of experimental investigation. The progress of science affords many warnings against trusting implicitly to the rule of continuity. Curves of change seldom preserve indefinitely a uniform character. Their unexplored sections may include quite unlooked-for peculiarities of flexure; and the possibility seriously undermines confidence in inferences depending upon "extrapolation." The presence of hydrogen and helium in our system cannot, then, be ranked among facts incontestably contradictory of the Nebular Hypothesis.

The concerted advance of mathematical astronomy during the eighteenth century was effected with the confident serenity of irresistible power. One after another, the obstacles barring its path went down before repeated and skilful onslaughts, the unbroken succession of which lends a certain exultant sameness to the story of the heroic age of analysis. The *Mécanique Céleste* attested "victory all along the line." There were no more worlds to conquer that Laplace knew of; the reign of gravitational law was firmly established throughout the solar dominions; menaced revolts had been appeased; anomalies removed; no extant observations any longer impaired the perfect harmony between what was and what had been anticipated. Nature for the moment submitted readily to the trammels put upon her by human thought; her intricacies no longer seemed to defy unravelment; her modes of procedure looked straightforward and intelligible. As they were judged to be in the present, so they might be presumed to have been in the past; and the temptation was irresistible to adventure backward speculation, inferring initial conditions from the elaborated product laid open to scrutiny.

It was an epoch of peremptory renewals. The formula of equality promised to regenerate society; a political panacea had been found by the creation of a Republic "one and indivisible"; and the success of the guillotine in securing its supremacy was almost outdone by the triumphs of the calculus in vindicating the unimpeded sway of gravitation.

Humanity had made a fresh start; science should do likewise. The sanguine spirit of a rejuvenated world animated all forms of human endeavour. It has long since evaporated. The buoyant hopes of a century back have been crushed; the future of civilization looks dim; the future of knowledge is compromised by its uncertainty. But we, at any rate, no longer delude ourselves with the idea that he who runs may read the secrets of the universe; we have learned by convincing experience how much, and how variously, "the subtlety of nature transcends the subtlety of sense and intellect"; we are vividly aware that there is no single and simple recipe for the "cosmification" of chaos.

That devised by Laplace has ceased to be satisfactory. Its simplicity, at first sight so seductive, leaves it at a disadvantage compared with the intricacy of the effects it was designed to elicit. The relations claiming explanation have multiplied with the progress of research. Those of the dynamical order were alone attended to by the geometers of the eighteenth century, and even they have grown recalcitrant; while those of a physical and chemical kind have proved wholly unmanageable. It has indeed become abundantly clear that the series of operations described by Laplace could scarcely, under the most favourable circumstances, have been accomplished, and in a thin nebulous medium would have been entirely impossible. The Nebular Cosmogony has not then stood "Foursquare to all the winds that blew."

Its towers and battlements have crumbled before the storms of adverse criticism. It survives only as a wreck, its distinctive features obliterated, although with the old flag still flying on the keep. In the next chapter we shall attempt a survey of the works set on foot for its reconstruction.

Letters.

[The Editors do not hold themselves responsible for the opinions or statements of correspondents.]

MAN'S PLACE IN THE UNIVERSE.

TO THE EDITORS OF KNOWLEDGE.

SIRS,—As I do not wish your readers to suppose that I have altered the words of an author whom I quote in order to make them agree more closely with my own opinions, will you permit me to state, in reply to Mr. Maunder's criticism of my article in the *Fortnightly Review*, that when that article was sent to press I had not seen Prof. Newcomb's book on "The Stars"; but I quoted from his "Chapters on the Stars," which appeared in *The Popular Science Monthly*. The quotation beginning "If we should blot out," is verbatim, as at page 323 of that periodical for January, 1901, except that I have, inadvertently, substituted "Milky Way" for "galaxy."

A writer in the *Daily News* of March 31st has so well answered Mr. Maunder's criticisms that it is unnecessary for me to refer to them here. I am, however, indebted both to Mr. Maunder and to Prof. H. H. Turner, who has criticised my article in this month's *Fortnightly*, because,

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although I consider their objections to be rather weak, and with one exception not much to the point, they are yet of great use to me, as showing me where my argument needs strengthening or where I am likely to be misunderstood.

In a volume I am now preparing I hope to be able to present my views in a more complete and more convincing manner.

Broadstone, Dorset, April 8th, 1903. ALFRED R. WALLACE.

TO THE EDITORS OF KNOWLEDGE.

SIRS,—Dr. A. R. Wallace's biological investigations have been elevated and illuminated, throughout their long course, by his strong appreciation of the incomparable moral dignity of Man. And this, too, is the inspiring thought of his recent article in the *Fortnightly Review*. It rises beyond discussion; welcome assent is the only fit attitude of mind towards it. But the profound significance of human life would be no whit impaired by assigning to the scene of its expansion an eccentric position in the universe; and a central position, as Mr. Maunder has shown in KNOWLEDGE, and Prof. Turner in the *Fortnightly Review*, can belong only momentarily to a moving body. Nor can it reasonably be supposed that the conditions of vitality deteriorate with remoteness from the centre. In the actual throng of the Milky Way, indeed, a peopled planet might be exposed to perils from "furious driving," or subtler obstructions to traffic. Within its ambit, however, there is "room and verge enough" for the unimpeded travelling of many millions of globes, freighted though they may be with superhuman destinies.

But has the universe a centre? In other words, is it of limited dimensions? Dr. Wallace avers that it is. His arguments are, nevertheless, contravened by the high astronomical authorities just referred to. The all-pervading illumination of the sky-ground, which should attest the shining of infinitely numerous suns, would be prevented, they agree in considering, by the intercepting action of an *equally infinite* number of dark bodies. These, however, cannot be supposed to screen off gravitational influences; they should, on the contrary, reinforce them. Hence, every body in space would be acted on by infinite forces, soliciting it to move in every direction at one and the same time. Cosmical paralysis would result; the "constellated suns" would stand stock still; unless we make the hazardous assumption that a finite attraction, super-added to an infinite sum of attractions, might avail to determine velocity. That is to say, neighbouring stars might exert effective pulls upon each other irrespectively of the equilibrating pull of a measureless universe.

There is, however, little profit in dwelling upon this baffling aspect of the question; clearer evidence is before our eyes. Everywhere in the heavens structural relations are manifest. Architectural design is traceable in them; they have parts fitted in together to form a vast yet limited whole. The Galaxy has shape and boundaries, and we have no assured knowledge of anything lying outside those boundaries. It seems, then, a gratuitous exercise of the imagination to conjure up abysses stored with stars, energetic and effete, one set cutting off the radiations of the other.

Undeniably, Dr. Wallace's contention that our earth is unique as being the abode of intelligent life corresponds, in a measure, with the recent trend of astronomical research. The conditions indispensable to organic existence which he has admirably defined are perceived, more and more plainly, not to be present on any of our sister-planets with the possible exception of Mars. Moreover, a large proportion of the stars have been ascertained to form systems unfavourably circumstanced for the accommodation of globes approaching the terrestrial model. Boundless variety, it is true, prevails throughout the sidereal scheme; and this variety may only afford scope for the display of the contriving power of Infinite Wisdom in smoothing away apparent obstacles to life, and so vitalising the seeming deserts of the universe.

AGNES M. CLERKE.

TO THE EDITORS OF KNOWLEDGE.

SIRS,—With reference to Dr. Wallace's article on this subject in the *Fortnightly Review* for March, 1903, I should like to make a few remarks.

As it is at present impossible to fix the exact limits of the visible universe we cannot determine where its centre may lie, and although the sun and solar system *apparently* lie near the centre of the Milky Way, it does not follow that we are really at or near that centre. Indeed, there is evidence to show that we are perceptibly nearer to one side of the Milky Way than to the opposite side. Sir John Herschel was of opinion that the southern portion of the Milky Way near the Southern Cross is nearer to us than the northern part. Professor Newcomb considers that we are probably nearer to the boundary of the visible universe which lies in the direction of Sagittarius and Scorpio, and he thinks that we may possibly be so much nearer this particular region that we may soon be able to detect proper motions among the fainter stars in this direction.

Considering the sun's motion through space as rectilinear, I pointed out ten years ago ("The Visible Universe," p 197) that at some period back in geological times the sun was probably among the stars of the Milky Way. The solar "apex," or point towards which the sun is moving, is, according to Prof. Kapteyn, in about R.A. 273°·6, North Declination 29°·5, and he thinks that "the most probable value that can at present be adopted" for the sun's velocity is 18·45 kilometres, or 11·46 miles a second. With this velocity, the sun's annual motion would be about 360 millions of miles (or nearly four times the radius of the earth's orbit). Hence the distance traversed in, say, 200,000 years would be 72 billions of miles. As the distance of Sirius from the sun may be taken as 50 billions of miles (parallax 0″·37), the solar motion carried back would place the sun far beyond the distance of Sirius 200,000 years ago. If we go back in geological times, say 5 millions of years, we find the sun at a distance of 1800 billions of miles from its present position in space. This would represent the distance of a star with a parallax of about 0″·01, or about 326 years' journey for light. Removed to this distance the sun would be reduced in brightness to a star of the 10th magnitude. It follows, therefore, that if the sun is moving in a straight line it must have been close to the position of 10th magnitude stars (if of the same size and brightness) some 5 millions of years ago. Going further back in geological time we should find the sun among the stars of the Milky Way. From the apparent connection of bright and faint stars in the Galaxy, Easton thinks that the faint stars of the Milky Way are at a distance which does not greatly exceed that of stars of the 9th and 10th magnitude (*Astrophysical Journal*, March, 1895).

It is of course possible, and indeed probable, that the sun is not moving in a straight line, but in some gigantic orbit round a centre of force. Recent researches seem to show that the centre of the Milky Way probably lies in a direction south of Cassiopeia's Chair, and a little south of the Milky Way (about R.A. 24h.), the sun and solar system lying to the south of the galactic centre, and a little to the

north of the plane of the Milky Way. Now the "apex" of the solar motion lies roughly 90° from this position, and judging from the position of the apex found by Sir William Herschel, Argelander and Airy (about 17h. 30m.), and that indicated by recent researches (about 18h. 30m.), there may perhaps be a shift of the apex towards the centre of the Milky Way, which should be the case if the sun were revolving round a centre. This supposed "shift" may of course be more apparent than real, and may, perhaps, be partly or altogether due to errors of calculation. The various positions, however, assigned to the apex show a tendency at least to shift towards the supposed centre of the Milky Way. However this may be, it seems not improbable that the sun may be revolving round the centre of gravity of the Milky Way which may also be the centre of gravity of the whole system of stars composing our visible universe. But if this be so, the sun is probably at a great distance from the centre. For its measured velocity is considerable, and in a system of stars, like a globular cluster, those near the boundary would have a greater velocity than those near the centre, the law of force in such a system varying *directly* as the distance from the centre.

J. E. GORE.

Dublin, April 14th.

TO THE EDITORS OF KNOWLEDGE.

SIRS,—Some further remarks on the question recently mooted between Dr. Wallace, on the one side, and Mr. Maunder and Dr. Turner, on the other, may be of interest to the readers of KNOWLEDGE. First, all the astronomical arguments in favour of a finite universe seem to me to turn on the assumption that the ether is infinite. Admittedly it is only by the light of very distant objects that we learn their existence; and for the transmission of this light, ether is necessary. Our knowledge is, therefore, confined within the limits of the ether, whether it be finite or infinite. But how can we draw any conclusion as to the finiteness or infiniteness of the universe if all that we can know of it is limited to the finite sphere of the ether? With an infinite and perfectly transparent ether, a limited number of shining bodies would be suggestive of a limited universe of matter; but if the ether itself be finite, we would, in drawing this conclusion, act like an islander who imagined that his island was the only bit of solid land in creation, because, when looking round on every side, he could see nothing but water. His universe would be limited by the curvature of the water as ours (on this assumption) is by the finiteness of the ether. But Dr. Wallace fails to see this. He speaks of bodies wandering outside the limits of the ether, but assumes that they would be cold and dead, &c. Why? If ether is necessary to convey heat to them, it is equally necessary to convey it away. They would, therefore, simply retain their heat without change. That they would be dark I admit, but light is not necessary to animal life. Then, if *our* ether be finite—if it be like a vast attenuated nebula—why may there not be other great nebulæ of ether in external space, some of them, perhaps, much larger and better supplied with stars than our own? A finite ether practically excludes us from knowing anything beyond its limits, and for that very reason it allows scope for all kinds of conjecture as to what is really beyond them. Dr. Wallace does not suppose it to be a vacuum. There are bodies in it which once belonged to our universe; and why should there not also be bodies in it that never formed part of our universe—never entered our ether? There are, at all events, no grounds for dogmatising on this subject. (But I do not think any observation has shown such appearances and disappearances of stars as would arise from their entering or leaving the ether.)

We are, according to Dr. Wallace, in the centre of the universe. He infers this from our position in relation to the visible stars. I need hardly say that when our knowledge of the visible stars is carried further, our present ideas as to their distribution may be largely modified, and even as it is I doubt if any astronomer could go within 1000 light-years (if I am to use that ill-selected unit) of the centre of the star-system as at present known. But so far as we know, while heat, light, and some other influences are dependent on the ether, gravitation is not, and whatever the distance may be its transmission is instantaneous. Hence if there are bodies outside of the ether, as Dr. Wallace supposes, these bodies are as influential in determining the centre of the universe as the bodies which are situated within the limits of the ether. No doubt the distance of these outlying bodies must be very great, but their number and mass may also be very great, and some of them may not be more distant than some of those within the ether but near its confines. A finite material world in an infinite ether is a hypothesis which finds some support in the facts of astronomy. A finite ether with matter (perhaps infinite) beyond it, is also to a certain extent a feasible hypothesis; but a finite ether and a finite material world having distinct and independent limits seems to me to be the worst hypothesis of the three, and is, moreover, one that does not assist us in fixing the centre of this finite universe of matter.

I therefore reject the astronomical basis of Dr. Wallace's theory, leaving it to others to deal with the conditions of animal life and mental development. I confess, however, that I have sufficient faith in the principle of evolution to think that man might accommodate himself to the conditions of life on almost any of the planets, provided that the change were sufficiently gradual, and a sufficient time were allowed to elapse. But some arguments used by Mr. Maunder and Prof. Turner seem to me also unsatisfactory. The first of these is the argument drawn from the number of dark bodies in space in answer to the proofs of finiteness deduced from the faintness of the general illumination of the sky. What would be the consequence if the dark bodies were on the average as numerous and as large as the bright ones, while the sun were adopted as an average specimen of the latter? Simply that the average illumination of the entire sky would be half that of the sun. The dark bodies would be as often behind the bright ones as before them. Now compare this with the fact. The highest estimate that I have seen of the total light of the full moon is $\frac{1}{300000}$ of that of the sun, the discs of the two bodies being about equal. Suppose that the dark bodies were 150,000 times as numerous as the bright ones, instead of being equal in number. Then the whole sky ought to be as bright as the illuminated portion of the moon. Everyone knows that this is not so. But it is said that the stars, though infinite, may only extend to infinity in particular directions, *e.g.*, in that of the Galaxy. Be it so. Where in the very brightest portion of the Galaxy will we find a portion equal in angular magnitude to the moon which affords us the same quantity of light? In the very brightest spot, the light probably does not amount to $\frac{1}{100}$ part of that of the moon (when full), thus raising the proportion to fifteen millions to one. Dark bodies may explain why the sky-light is not infinite—which in fact it could not be even if all bodies were bright, and their number infinite—but the extremely small amount of sky-light which actually exists is a startling fact, and one difficult to reconcile with the theory of an infinite material universe.

Again, I think too much stress has been laid by Dr. Wallace's opponents on the motion of the solar system in space. In the first place, the proof of this motion rests on

the assumption that if we take a sufficient number of stars their real motions in all directions will be equal, and that, therefore, the apparent preponderances which we observe in particular directions result from the real motion of the sun. But there is no impossibility in a systematic motion of the majority of the stars used in these researches which might reconcile the observed facts with a motionless sun. And, in the second place, if the sun is not in the exact centre of gravity of the universe, we might expect him to be moving in an orbit around this centre of gravity, and our observations on his actual motion are not sufficiently numerous or accurate to enable us to affirm that he is moving in a right line rather than in such an orbit. Indeed, the positions assigned to the apex of the sun's way appear to show a steady change as we use stars with diminishing proper motion which are presumably at increasing distances, and though other explanations of this circumstance may be given, one admissible hypothesis is that the different directions thus computed represent the tangent to the sun's orbit at different dates, and thus establish its curvature. To anyone, however, who realises the vast dimensions of the known universe, and the manner in which science is constantly opening out great new vistas into the unknown, a hypothesis like that of Dr. Wallace will appear to be exceedingly improbable, and I do not think the basis of observation on which it rests is sufficiently extensive and solid to justify its acceptance by the votaries of science.

I may remark that if, as I have suggested, the ether absorbs light, we have an explanation of many of the difficulties which arise in connection with this question. But apart from this hypothesis, meteors in space—cosmic dust—may absorb a sensible proportion of the light of luminous bodies, this proportion increasing with the distance. We could not discover this fact within the limits of the solar system, because the planets shine by reflected light, and the meteors or cosmic dust which intervened would reflect the light also, and being nearer to us than the planet would, in fact, increase its light, provided that their reflecting power were the same. The effect of their interposing between us and a self-luminous body would be quite different, and if they exist throughout all space, the greater the distance the more of the light they would (on the average) intercept. If sufficiently remote, the star would thus, for all practical purposes, be blotted out.

I may add that on the assumption of a finite ether forming an oasis in a desert universe, which seems to be Dr. Wallace's idea, not only is it unlikely that the centre of the universe would fall within the ether, but even if it chanced to do so, the ether would probably have a proper motion of its own which would soon carry it away from this centre. Further, assuming that life and intelligence could only be developed within the limits of this ether, and (as Dr. Wallace suggests) that the development would therefore be carried farthest in the case of the bodies that had been longest in it, I would expect to find these bodies not in the centre, but near the confines, at the opposite side from that at which they had originally entered it.

W. H. S. MONCK.

British Ornithological Notes.

Conducted by HARRY F. WITHERBY, F.Z.S., M.B.O.U.

The Meadow Bunting (Emberiza cia) in England.—At the meeting of the British Ornithologists' Club, held on January 21st last, Dr. R. B. Sharpe reported that he had recently examined a specimen of this bird caught near Shoreham, Sussex, at the end of October, 1902. There were two of these birds captured out of a flock of chaffinches and other birds, one had died and the other was in the possession of Mr. E. A. Hackett, of East Finchley. The Meadow Bunting is an inhabitant of Central and Southern Europe and Asia Minor, and has not been recorded before for Great Britain.

Great Bustard in Ireland (*The Field*, March 14th, p. 447).—Messrs. Williams & Son, the Dublin taxidermists, report the occurrence of two Great Bustards near Thurles, Co. Tipperary, in December, 1902. One of the birds (a female) was shot on the 20th of that month. The Great Bustard has not been recorded before for Ireland.

White's Thrush in Yorkshire (*The Naturalist*, March, 1903, p. 68).—A specimen of White's Thrush (*Turdus varius*) was shot in Luddenden Dean, near Halifax, on December 18th, 1902. The bird is now in the Halifax Museum, and a full account of it, illustrated with a photograph, appears in the *Halifax Naturalist* for February, from the pen of Mr. A. Crabtree. This bird is somewhat larger than a Mistle-Thrush, is mottled in plumage, and is generally to be found on the ground. It is an eastern species, and has seldom been recorded from Great Britain.

Lesser Whitethroat breeding in West Ross-shire (*Annals of Scot. Nat. Hist.*, April, 1903, p. 71).—Messrs. L. W. Hinxman and W. E. Clarke have identified a nest and eggs taken at Inverbroom, West Ross-shire, in 1896, as those of the Lesser Whitethroat. This bird breeds rarely in S.W. Scotland, but has hitherto only been recorded as a passing migrant elsewhere in Scotland.

Rough-legged Buzzard in Ireland (*Irish Naturalist*, April, 1903, p. 111).—Mr. E. Williams records that a female specimen of *Buteo lagopus* was recently shot near Fivemiletown, Co. Tyrone. This bird is a rare visitor to Ireland.

The Status of the Goldfinch in Britain (*Zoologist*, 1903, February, pp. 70–72, March, pp. 104 and 105).—Observers in various counties continue here their opinions on the status of the Goldfinch in Great Britain.

Bean-goose in Outer Hebrides (*Annals of Scot. Nat. Hist.*, April, 1903, p. 119).—Mr. J. A. Harvie-Brown records that on March 21st last he received a specimen of this Goose for examination. The bird was sent from South Uist, and two others were observed at the same time. In the last edition of his "Manual of British Birds," Mr. Howard Saunders remarks that reported occurrences of this species in the Outer Hebrides, Orkneys and Shetlands, require confirmation.

British Bean-geese. By F. W. Frohawk. (*Zoologist*, 1903, February, pp. 41–45.)—Mr. Frohawk here reverts to the discussion of the Bean-geese (*see* KNOWLEDGE, November, 1902, p. 255).

On the specific validity of Anser rubrirostris (Hodgson) *and its position as a British Bird.* By F. Coburn. (*Zoologist*, February, 1903, pp. 46–52.)—This is a somewhat long and detailed description of the eastern form of the Grey Lag-goose obtained by Mr. Coburn from Limerick (*see* KNOWLEDGE, October, 1902, p. 231). Mr. Coburn is apt to become somewhat elated, and decidedly dogmatic, when on the subject of Geese.

On the Avifauna of the Outer Hebrides, 1888–1902. By J. A. Harvie-Brown.—Mr. Harvie-Brown sends me a most useful reprint from the *Annals of Scottish Natural History* of his series of articles, which bring the avifauna of the Outer Hebrides up to date.

Supposed Breeding of the Hawfinch in Ireland (*Irish Naturalist*, October, 1902, p. 250, and April, 1903, p. 111).—The Hawfinch has never been proved to nest in Ireland although it is often found there in winter. Notes have lately appeared, however, in the *Irish Naturalist*, stating that Mr. Bedford has seen, near Straffan House, Co. Kildare, a few pairs of these birds all through the nesting seasons of every year since 1896, and that last year he saw a Hawfinch feeding its young.

All contributions to the column, either in the way of notes or photographs, should be forwarded to HARRY F. WITHERBY, ***at the Office of*** KNOWLEDGE, 326, ***High Holborn, London.***

Notes.

ASTRONOMICAL.—The most interesting recent event in astronomical matters is the discovery of a "new star" at the University Observatory, Oxford. On March 16th, in the ordinary routine of the work connected with the photographic chart of the heavens, a plate was exposed on a part of the constellation Gemini, and it happened that the new star was accidentally selected for the guiding star in place of the one intended. Subsequent examination of the plate indicated "an error in setting," and led to the recognition of the star as one not previously catalogued. The R.A. and Decl. are respectively 6h. 37m. 48·9s. and + 30° 2′ 39″ (1900). Photographs taken on February 24th, and earlier, did not show the star; on March 16th its magnitude was 7, and on March 27th it was 8·5. Observations by Mr.

Newall, Prof. Hale, and others, indicate that the spectrum consists of bright lines, and there is accordingly sufficient reason to suppose that the object is a true "nova," and not a long period variable of the Mira type.—A. F.

BOTANICAL.—In *Torreya*, Mr. S. B. Parish gives a striking instance of the "Vital persistency of *Agave americana*" A specimen was planted on an estate in California in 1890, and having become too large, was cut down in 1900. Its leaves were sawn off and the trunk was allowed to lie on the ground till it should become sufficiently dry to burn. In May of last year, rather more than two years after being cut down, it put forth a flowering scape which grew, in its early stages, at the rate of six inches a day, till it reached a height of fifteen feet. This scape flowered, afterwards bearing fruit, and in November of the same year, three smaller scapes were produced. The author comments on the curious fact that this vigorous growth, remarkable as it would have been had it taken place in the first year, was deferred till the second year after the plant was cut down.

A flora of New Providence and Andros, Bahama Islands, appears in the last number of the *Memoirs of the Torrey Botanical Club*. It is based on a set of plants collected in the islands by Mr. and Mrs. J. I. Northrop, and is the work of Mrs. Northrop, assisted by various botanists. New Providence is one of the smaller islands of the group, while Andros is by far the largest, being nearly a hundred miles long and forty or fifty wide in its broadest part. The enumeration, which is preceded by an interesting introduction, includes 542 native species. Leguminosæ are most numerous, followed by Compositæ, Rubiaceæ and Euphorbiaceæ, in the order here given, and *Ipomœa* is the richest genus in species. Two new genera of palms are described by Mr. O. F. Cook, and miscellaneous new species, including a new *Vanilla*, are now published. It is worthy of note that fifteen genera are found in the enumeration which have not previously been reported from the Bahamas.—S. A. S.

ZOOLOGICAL.—Remains of man-like apes are of such rare occurrence in the fossil state, that the description of two molar teeth from the Tertiary deposits of the Vienna basin by Dr. O. Abel (*Centralblatt für Mineralogie, &c., 1903*), is a matter of very considerable interest. The one specimen is referred by the author to a new genus and species, under the name of *Griphopithecus suessi*, while the other is regarded as representing a new species of the well-known genus *Dryopithecus*, for which the name *D. darwini* is suggested. As regards the first, all that can be said is that it indicates a member of the group to which *Dryopithecus* belongs. By some writers it has been suggested that the latter genus is very closely related to man, but this idea is discountenanced by the great relative length of the muzzle and the small space for the tongue. It may be added that recently teeth of another man-like ape from the Tertiary of Swabia were described under the name of *Anthropodus*; since, however, that term had been previously used in another sense, Dr. Abel proposes to replace it by *Neopithecus*.

Closely connected with the foregoing subject is a paper by Prof. A. Gaudry, of Paris, published in *L'Anthropologie*, on certain human remains recently obtained by the Prince of Monaco at Baoussé-Roussé, near Mentone. The jaws of a young man are remarkable for the enormous size of the teeth, especially those of the cheek series, and the great relative development of the last molar or "wisdom tooth." That they indicate a very primitive type of the human race is undoubted. Whether, however, they are sufficient to prove, as M. Gaudry suggests, an affinity between the early human inhabitants of Europe and the Patagonians, and thus that South America was the cradle of mankind, demands at least further consideration.

Much interesting information with regard to the giant land-tortoises, and the remarkable land and sea iguanas of the Galapagos Islands will be found in a paper by Mr. E. Heller, recently published in the *Proceedings* of the Washington Academy of Sciences, forming part of a series descriptive of the Hopkins Stanford expedition to the islands in question. As might have been expected, most of the species of tortoises are diminishing very rapidly in numbers, and some appear to be well nigh extinct. Owing to the introduction of dogs, the land iguanas have been exterminated from all the islands save two, but the marine species is still abundant.

In view of forthcoming changes in the administration of the menagerie in the Regent's Park, the following paragraph from the Report of the New York Zoological Park is of considerable interest. In that menagerie one of the most recent and important innovations is the establishment of a thoroughly organized medical department under the direction of a veterinarian and a well-known human pathologist. A pathological laboratory is in charge constantly of an assistant, and daily rounds are made by an officer of the medical staff in company with the curators of the respective departments. Full reports are being kept of the symptoms of animals of various types, and of the causes of death. From these records it is proposed to prepare a special work on the habits, care and treatment of animals in captivity.

THE BRITISH ANTARCTIC EXPEDITION.—A summary of the news of the "Discovery" received by cable from Captain Colbeck, of the "Morning," may be of interest to our readers. The "Discovery" left New Zealand in December, 1901, and the "Morning" found her in MacMurdo Bay (Victoria Land), on January 23rd, 1903. The "Discovery" entered the ice pack early in January, 1902, in lat. 67° south. Proceeding along the edge of the great ice barrier, the explorers made repeated soundings. It was found that the barrier was presumably afloat, that it continued horizontal, and was slowly fed from the land ice. High snow slopes rose from its edge to an extensive heavily glaciated land, with occasional bare precipitous peaks. On February 3rd, the "Discovery" entered an inlet in the barrier in longitude 174°, and a sledge party examined the land as far as latitude 78° 50′. Near Mounts Erebus and Terror excellent winter quarters were found. The ship was frozen in on March 24th, and the expedition passed the winter in well-sheltered quarters, huts having been erected for living and for scientific observations. The lowest recorded temperature was 62° below zero, and the fact that the expedition wintered some 400 miles further south than any former expedition will make the magnetic and other records taken of very great value. Sledging was begun on September 2nd, 1902, parties being sent out in various directions. Lieutenant Royds, Mr. Skelton, and others made an expedition to Mount Terror, travelling over the barrier under severe sleighing conditions, with a temperature of 58° below zero. Commander Scott, Dr. Wilson, and Lieutenant Shackleton sledged southwards. A depôt was made 60 miles south of the ship, then the snow became soft, and sledges had to be hauled in relays. This hard and trying work lasted 29 days. Another depôt was then established in latitude 80° 30′ south. All superfluous gear was then discarded, and on December 15th a dash was made for the south. On January 1st, 82° 17′ south was reached, or 100 miles

further south than any previous record. The conditions were very trying, the dogs all died, and the sledges had to be dragged back to the ship, which was reached on February 3rd. The party found that ranges of high mountains continue through Victoria Land. Mountains 10,000 feet to 12,000 feet high were seen in latitude 82° south, and the coast line continued at least as far south as 83° 20′, so that it seems that land stretches to the South Pole in a series of lofty mountains. Further details of all this successful work, and of the many collections and observations made, will be eagerly looked for, and it is hoped that an equally useful year's work will be accomplished by the expedition in 1903. The "Discovery" has been revictualled by the "Morning," and will winter again in the Antarctic. Lieutenant Shackleton has returned in the "Morning" owing to his health, which suffered greatly owing to the privations and hardships of the sledge journey. Lieutenant Mulock has taken his place.

Earthquake in the Midlands.—The earthquake felt throughout the Midland counties on March 24th was not one of the first magnitude, even for this country. It resulted in but little damage to buildings; though its strength only fell short of that necessary for throwing down a few chimneys or cracking the walls of houses. The area disturbed by it includes the whole of Derbyshire and Staffordshire, and a very large part, possibly all, of the adjoining counties. The shock was felt as far north as Preston, Lancaster, Leeds and Harrogate; to the east, it was perceptible at Grantham, and to the south, it is said, at Evesham; that is, over an area of about 13,500 square miles. The position of the epicentre is not yet determined, but it will probably be found to be at no great distance from that part of Dovedale which lies west of Matlock. British earthquakes seldom last more than ten or fifteen seconds, but the duration of the recent shock must have been over, rather than under, half a minute. This was due to its division into two distinct parts, separated as a rule by an interval of a few seconds. On this point, the personal experience of observers is corroborated by seismographic evidence. An interesting record of the earthquake was given by one of Prof. Omori's horizontal pendulums installed in Birmingham, and shows clearly two large oscillations, the total duration of the shock, including the initial and final tremors, being about one minute. The earth-waves, according to Dr. E. Wiechart, were also registered at Göttingen, which lies about five hundred miles from the centre of disturbance.—C. D.

Notices of Books.

"Problems in Astrophysics." By Miss Agnes M. Clerke. (A. and C. Black.) Illustrated. 20s. net.—It is the custom in England, in every great business firm, to go over the accounts periodically, to balance the gain and loss, to take stock of the assets of the company. The principle of a "periodical stock-taking" is no less a sound one when applied to a science than to a business concern, but it is less often put in practical working; and we feel proud that it is an English astronomer who has undertaken and carried through the audit for the most exact of all the sciences.

Miss Clerke's "Problems in Astrophysics" is indeed a great astronomical stocktaking. She lays plain before us the capital of our knowledge. She shows what are the sound branches from which advancement and profit may be expected. She exposes the unsound principles whose practice can only mean disaster, the theories so rigid and complete that they cannot expand to include new facts, and so impede progress. "Finality means stagnation." She writes down unsupported speculation as of no value whatsoever. "The 'floating of an idea' in the mind does not constitute knowledge; and a speculation is only valuable when it offers a definite starting point for practical research." She discounts any complex hypothesis "which raises more difficulties than it removes."

The book is divided into two parts. The second deals with the problems of sidereal physics; with those presented by various types of stars; with variable stars of all kinds; with spectroscopic binaries; and with nebulæ, "white" and "green." Our sun is a true member of the sidereal system, but from his proximity to us we can study him in detail as we cannot remoter suns, and so another series of problems connected with the solar surface and surroundings is presented in the first part. Though these two parts are for convenience treated separately, essentially the same problems enter into both, and a question in the one may find its answer or its corollary in the other.

In the first part it may be said that all problems lead up, more or less directly, for their solution to two questions. What is the cause and law of the sun's periodicity? What are the natures and laws and conditions in the sun's surroundings? These two questions are not distinct, they are bound up with each other, and the answer to one will certainly throw light on, if it does not completely solve the other. And is the first question solvable? Probably, if the law of periodicity be due to an external cause, but Miss Clerke believes it is not so due. The cyclical changes of the sun may be inherent, or "may simply characterise a stage of growth, and prove liable to modification and effacement. The study of variable stars will perhaps help to guide ideas as to the probabilities of the case." So Miss Clerke states the case and leaves it. It is indeed a hard problem to pierce below the sun's photosphere and understand the conditions imposed in a state so unattainable in our earthly experience. And here we must express regret that Miss Clerke has omitted all discussion of both planets and comets on the plea that they "belong to the theoretical and descriptive departments of the elder celestial science." In Jupiter we have a state of things that bears directly on the question of the solar surface markings, of their periodicity, and of their connection with rotation.

In the second part, Miss Clerke assumes that time is the principal factor in the evolution of the type of a star; other factors, such as mass, modifying but not determining the type. The order of succession of the four chief families, she says, leaves little room for doubt. Nebulæ are the matrices of stars; of these are, first, helium stars; then these pass by fine gradations into Sirian: the Sirian by turn into solar; and these again into stars with fluted spectra. It is assumed that photospheres of helium and Sirian stars are "unveiled," and that those of solar are "veiled," but this seems scarcely axiomatic; similarly the assertion, on Dunér's authority, that the absorption in sun-spots is gaseous, namely that there is no increase in "general absorption" in the umbra, is by no means proved to hold good at all times and in all spots. By variable stars, the question of the sun's periodicity is presented again in a more acute form, and the constitution of "green" nebulæ raises many points that urgently call for elucidation from laboratory experiments.

It is a veritable *tour de force* to have brought out two such books as the fourth edition of "The History of Astronomy in the XIXth Century," so largely a new work, and "Problems in Astrophysics" within a few months of each other. We must confess to having opened the second of the volumes with a certain amount of trepidation; the first was of such a high order that it did not seem possible that Miss Clerke's strength would permit her in so short a period to produce a work of even equal excellence. But of her three great works, "The System of the Stars," "The History" and "Astrophysical Problems," we can unhesitatingly affirm that the latest is of even more value and is the outcome of even profounder thought than its two predecessors. There is one defect for which the publishers are to blame. Miss Clerke's book is emphatically one that will *not* be placed in that bookcase which is never opened. Is it right to issue such a standard book with its pages uncut, so that when the paper knife has done its work, the edges form an ideal lodging for the dust and microbes of the world?

"An Introduction to Celestial Mechanics." By F. R. Moulton, Ph.D., Instructor in Astronomy in the University of Chicago. (Macmillan.)—This work will be of very great assistance to those students of Formal Astronomy (to use a particularly happy phrase of Dr. Moulton) who wish to connect mechanical and physical causes with their observed phenomena. In a book numbering less than 400 pages, and dealing with a subject so wide and so abstruse, it has been necessary to assume

that the reader knows much concerning the questions, and that he is acquainted to a considerable extent with mathematical processes. But, granted that, the working out of the various problems is presented with clearness and judgment. Dr. Moulton deals with rectilinear motion, with the problem of two bodies, with the problem of three bodies, with various perturbations, and with the theory of determinations of the elements of parabolic and elliptic orbits. Amongst many interesting details we may mention one in particular in connection with the Gegenschein. In the problem of three bodies—the sun, the earth, and a third body infinitesimal as compared with either—assume the origin of co-ordinates at the centre of mass and the axis of x as the moving line joining the circles of the sun and earth, both of which are supposed to move in centres round their common centre, the plane of x y being the plane of their motion. Assuming that the infinitesimal body is also moving in this plane, Dr. Moulton finds the locus referred to the rotating axes, of those points in space where the velocity of the particle is zero. On this locus there are three double points on the x-axis, one lying between the sun and earth, and one on each side of these two bodies. Assuming the circularity of the earth's orbit, the double point which is opposite the sun is distant from the earth 930,240 miles, and at this point an infinitesimal body should just escape eclipse. If the infinitesimal body should arrive near this point under initial conditions (defined by equations too long to quote here) it will revolve in an ellipse round the point of equilibrium, and the major axis of the ellipse lies along the axis of y. If the initial conditions are very approximately complied with, the small body will move in an ellipse for a considerable time, but will eventually depart far from it, and it would be possible to have any number of infinitesimal bodies revolving around the same point without disturbing each other. If, therefore, a great number of meteors were moving in the plane of the earth's motion under the required approximate initial conditions, they would appear as a hazy patch of light with its centre at the anti-sun and elongated along the ecliptic, and this agrees substantially with the observations of Backhouse and Barnard. If Dr. Moulton's solution is the true explanation then the Gegenschein is not closely connected with the Zodiacal Light. True, Dr. Moulton says: "It is certain that the meteors are exceedingly numerous, as many as 8,000,000 striking into the earth's atmosphere daily according to the late Professor H. A. Newton, and it is only reasonable to suppose that they cause the Zodiacal Light, which is very bright compared with the Gegenschein." Dr. Moulton does not, however, give his reasons for assigning the origin of the Zodiacal Light to meteors, and we do not see that it follows from any one of the propositions here discussed.

"THE TWENTIETH CENTURY ATLAS OF POPULAR ASTRONOMY, COMPRISING IN TWENTY-TWO PLATES A COMPLETE SERIES OF ILLUSTRATIONS OF THE HEAVENLY BODIES." By Thomas Heath, B.A., First Assistant Astronomer, Royal Observatory, Edinburgh. (W. & A. K. Johnston, Limited, Edinburgh and London. 1903.)—This very handsome volume is, so far as its plates are concerned, essentially a new edition brought up to date, of Hind's Atlas, so popular forty years ago. The letterpress is largely new, and is due to Mr. Thomas Heath, First Assistant at the Royal Observatory, Edinburgh. Taking the plates first, the old ones were very admirable at their original date, and were finished with a care much greater than was at all usual at that time. But it may be questioned whether we are not now in a position to exact a higher standard. Plate XII. (views of nebulæ) is by no manner of means satisfactory. Apparently, the blue ground has proved utterly unsuitable for the reproduction of nebular photographs, and this Plate in consequence is worse than misleading. Plate X. (of comets) is not quite so bad, but here the most recent comet given is that of 1882. There have been many since that date more significant as to their structure than any shown here. Mr. Heath has fulfilled his difficult duty of bringing the letterpress up to date with considerable skill, but every here and there a little point suggests an oversight in the revision, or at least an incompleteness. To take a few instances:—The only reference as to the contour of sunspots is to Prof. Wilson's theory, illustrated by a rather crude diagram; Sir George Airy's determination of the density of the earth is the most recent one given; the chapter on meteors contains no reference to anything later than the shower of 1866, save for the fall of a single meteorite in 1890; the Zodiacal Light is "supposed to be due to sunlight reflected from minute meteoric particles crowding the region between the sun and the orbit of Mercury." Still, with all these imperfections, the book is a handsome and a useful one. Chapters X. and XI. on the moon and eclipses are especially well illustrated.

"WAVES AND RIPPLES IN WATER, AIR AND ÆTHER." By Prof. J. A. Fleming, F.R.S. Pp. 299. Illustrated. (S.P.C.K.) 5s.—The title of this book comprehends the most important branches of physics; for, according to modern conceptions, wave motion accounts for the greater part of physical phenomena. Waves and ripples in the air lead to sound and music, and in the æther they give rise to light and electricity. Prof. Fleming had therefore a wide subject to deal with when he gave the lectures at the Royal Institution, which are reproduced in the volume under notice. Everyone who sees this book will be glad that the lectures are now available to a wider audience than that of Albemarle Street, for they are full of interest and instruction. The style is easy and pleasant to read, the experiments and other examples are striking, and the illustrations are clear and attractive. Prof. Fleming knows how to deal with a subject in a manner which claims attention and commands respect for the dignity of science; for this alone his book merits many readers. No better popular yet scientific account of the principles upon which wireless telegraphy is based could be desired than this volume affords, and among similar subjects effectively dealt with are the electron theory of electricity, the relations between light and electricity, motions of vessels in relation to waves in water, experimental tanks for the study of motions of ships of various designs, earthquake waves and the phonograph. Every page contains an interesting statement of fact or principle, and we urge all who desire to enrich their knowledge to obtain a copy of Prof. Fleming's book.

"PUBLICATIONS OF THE MAHARAJA TAKHTASINJI OBSERVATORY, POONA." Vol. I. "REPORT ON THE TOTAL SOLAR ECLIPSE OF JANUARY 21-22, 1898, AS OBSERVED AT JOUR, IN WESTERN INDIA." By Kavasji Dadabhai Naegamvala, M.A., F.R.A.S., etc., Director of the Observatory.—It is nearly five years since the total solar eclipse occurred, the observations of which Prof. Naegamvala here reports. This delay is certainly much to be regretted, since two important eclipses have taken place in the interval, but in all probability the delay has arisen from causes quite beyond Prof. Naegamvala's control. The report bears strong evidence to Prof. Naegamvala's energy and power of organisation in bringing together and equipping so well-ordered a force for the observation of the eclipse. The most important work which he undertook was spectroscopic, and his most powerful instrument a Cooke triple achromatic, of six inches aperture, used with two prisms of 45° as a prismatic camera, and a series of successful photographs were taken with it. Of these, the first plate, taken at the instant of second contact, appears of most importance, and a long list is given of the bright lines measured upon it. This plate was specially exposed for the purpose of the study of the "Flash," and it is disappointing to find that Prof. Naegamvala does not venture to draw any conclusion as to the relation of the "Flash" to the Fraunhofer spectrum. He quotes, indeed, at length Mr. Maunder's statement of the points at issue from KNOWLEDGE for August, 1898, but has nothing to say as to the light which his results can throw upon them. With some of his smaller spectrographs he obtained important spectra of the corona, beside photographs of the corona itself with various instruments. A fine composite drawing of the corona from the latter photographs made by Mr. Henry Cousens, Superintendent of the Archæological Survey, to whom several of the plates of spectra, and some very interesting studies of coronal types are also due, forms the frontispiece to the volume.

"THE FLORA OF THE EAST RIDING OF YORKSHIRE, INCLUDING A PHYSIOGRAPHICAL SKETCH." By Jas. Fraser Robinson. (Brown & Sons. 1902.) 7s. 6d.—The North Riding of Yorkshire has had an able botanical exponent in Mr. J. G. Baker, and the West Riding in Dr. Arnold Lees. Now the East Riding has also a flora of its own, thanks to Mr. Robinson and the Members of the Hull Scientific and Field Naturalists' Club. Among the many local floras published of late years the present book will take a high place. There is throughout a good sense of proportion; the general features

XUM

are ably dealt with, and the work is not weighted with an unwieldy mass of detail. The introductory matter is especially well worked out. The geology and physiography of the area are described clearly and well, and their relation to the flora fully set forth. The flora is analyzed according to petrological conditions, and also according to water supply and nature of soil. The history of the progress of research in the flora of the East Riding is sympathetically treated. A condensed account of local meteorology is also given, sufficient for the needs of the botanist. It is stated that "alien plant names are always in italics" in the body of the book, but this salutary rule is not invariably carried out, and certain aliens and casuals appear in the same type which is accorded to native species. The "incognita," too, which include some records admittedly erroneous (*e.g.*, *Carex Davalliana*), are printed without any distinguishing mark as regards type, which is hardly satisfactory. But these are minor blemishes.

"NIELS HENRICK ABEL: MEMORIAL PUBLIE À L'OCCASION DU CENTÉNAIRE DE SA NAISSANCE." Edited by Elling Holst, Carl Stormer, and L. Sylow. (Kristiania: Jacob Dybwad. London: Williams & Norgate. 1902.)—Now that Abel's great mathematical discoveries have been universally acknowledged, it is as easy as it is useless to sing his praises. Let us rather avail ourselves of the story of his life, as told by M. Elling Holst in this fitting commemoration of the centenary of Abel's birth, and take a lesson from it. Instead of a man on whom fortune smiled, we find a young lad with a passionate enthusiasm for mathematics, struggling in the face of adversity, whose academic *Alma mater* denies him the bare means of subsistence, whose career is a short but hard fight against ill fate. Both at school, after his mathematical ability had been discovered by Holmboe, and at the University of Christiania, his life is on the whole a happy one, and at the end of his academical *curriculum*, he obtains a scholarship to enable him to travel abroad for two years. But before that time has elapsed, the chair of mathematics, which he of all men is best qualified to fill, has been awarded to his old teacher, Holmboe; the experienced teacher, as so often is the case, being preferred to the rare genius. On his return home, he seeks to obtain the bare means of subsistence while he is carrying on his important researches, but his appeal to those in authority meets with a curt refusal, and it is only by repeated applications that he obtains a pittance less than that of any other "privat-docent," indeed, as M. Holst puts it, "too small to live or die on." From Crelle, in Berlin, he has received every encouragement. Crelle publishes his works and will gladly find employment—such as he can offer—for him. At Paris, he as a foreigner has met with but little success, and his paper containing the epoch-making discovery known as Abel's theorem has been laid aside and neglected by those to whom it was confided for publication. Instead of remaining under Crelle he considers it his duty to return to his own university, where he becomes a cat out of kind, a sort of *homme incompris*. His extraordinary genius finds no opening in an academic body, the ruling powers of which might be described as good but not exceptional men. The discovery that another worker, Jacobi, is developing a theory of elliptic functions on parallel lines to Abel's, combined with the hardships he had undergone, no doubt tended to bring about the illness by which Abel's career was terminated, two days before his good friend Crelle wrote definitely informing him of his appointment to a chair at Berlin. This was in April, 1829, only three and a half years after the commencement of Abel's foreign expedition.

Now what makes the story of Abel's life the more painful is the thought that a man of Abel's type would fare no better at an English university at the present day than Abel himself did at Christiania nearly three-quarters of a century ago. Like Abel, he would find plenty of "kind friends" who would tell him that his work would secure for him a world-wide reputation, but they would not raise a hand to give him substantial assistance. They would counsel him to continue his researches and to wait on, because "an opening for him would be sure to occur very soon," but when the opening did occur, they would vote for the man of mediocre ability as against the genius. If Oxford and Cambridge do not possess Abels, they at any rate train mathematicians and others up to the stage when they may be competent to undertake research work that is useful and interesting. That these men are advised to remain at the University with the prospect of obtaining appointments, which are afterwards refused them and allotted to others, is an experience far more common than is generally known. The practice is a mischievous one which cannot be too severely condemned, and it is to be hoped that those who are in danger of falling victims to it will take warning by the fate of Abel.

BOOKS RECEIVED.

Encyclopædia of Accounting. Vol. I. Edited by George Lisle, C.A., F.F.A. (Wm. Green & Sons.)

The Soil. By A. D. Hall, M.A. (Murray.) 3s. 6d.

Photographic Lenses. By Conrad Beck and Herbert Andrews. (R. & J. Beck, Limited.) Illustrated. 1s. net.

Agricultural Geology. By J. E. Marr, M.A., F.R.S. (Methuen.) Illustrated. 6s.

Open-Air Studies in Bird Life. By Charles Dixon. (Griffin.) Illustrated. 7s. 6d.

Laboratory Guide for Beginners in Zoology. By Clarence Moores Weed, D.Sc., and Ralph Wallace Crossman, B.A., M.Sc. (Heath.) Illustrated. 2s. 6d.

Guide to the Early Christian and Byzantine Antiquities. (British Museum.) Illustrated. 1s.

Country Gentlemen's Estate Book, 1903. (Country Gentlemen's Association, Limited.) 5s. net.

School Geometry. Parts I. and II. By H. S. Hall, M.A., and F. H. Stevens, M.A. (Macmillan.) 1s. 6d.

Reports on the Administration of Rhodesia, 1900–1902. (British South Africa Co.)

Transactions of the Royal Scottish Arboricultural Society. Vol. XVII. Part 1. (Edinburgh: Douglas & Foulis.)

Zoological Gardens, Giza, near Cairo, Report for the Year, 1902. By Stanley S. Flower, Director. (Cairo: National Printing Department.)

Wireless Telegraphy and Telephony. By Dr. Maurice Ernst. (Rentell.) 1s.

Rhodesia Museum, Bulawayo. Annual Report, 1902; and Special Report—The Zimbabwe Ruins. By F. P. Mennell, F.G.S.

19th Annual Report of the Bureau of American Ethnology, 1897-98. Parts 1 and 2. By J. W. Powell. (Washington: Government Printing Office.)

Tsimshian Texts. By Franz Boas. (Bulletin 27, Smithsonian Institution.) (Washington: Government Printing Office.)

Human Origins. By Samuel Laing. (Watts & Co.) 6d.

Leeds Astronomical Society—Journal and Transactions during the year 1902. (Wesley.) 2s.

Cambrian Natural Observer, 1902. Edited by Arthur Mee. (Cardiff: Evans & Williams.)

Symons's Meteorological Magazine. Edited by Hugh Robert Mill, D.Sc., LL.D. April, 1903. (Stanford.) 4d.

Lenses for Photography. (Taylor, Taylor & Hobson, Ld.) Free.

Photographic Catalogue. (Jas. Woolley, Sons & Co., Ld., Manchester.)

CLOUDS AS SEEN FROM CLOUD LEVEL.

By Rev. JOHN M. BACON, F.R.A.S.

IN aerial travel there is often some little difficulty in determining what is actual cloud level; and in consequence it is not always easy to form a correct idea of the true aspect of clouds. If cloud be lying beneath an observer it may be quite impossible for him to judge by the eye whether the upper limit be a hundred feet or a thousand feet, or even twice that distance below him; and this will make all the difference in the estimate that is made of the true form and composition of the cloud-mass. No doubt the remarkable transparency of aerial space at high altitudes, and the absence of any intermediate objects to aid the eye in forming a right judgment of scale or distance is the chief cause of the difficulty referred to. Kinchinjunga, as seen in exceptionally transparent air from Darjeeling, often seems to the eye scarcely more than a mile distant, though in reality it is forty, and obviously the true character of that mountain's slopes will be more or less accurately described by an observer according as its true distance is duly estimated.

The accompanying group of photographs show the strikingly different aspects of the upper surface of a cloud-mass as seen from different heights. The first photograph

was taken practically at the cloud level, the second at about a thousand feet above, and the third at fully a mile above the limit of the cloud. Yet even in this last case the cloud appeared but as an extended floor lying only a few feet below the car; and as judged by the eye it seemed inexplicable how the trail rope, a hundred yards long, was not immersed in it. Thus anyone who had not tested the depth of the intervening distance and describing simply what he appeared to see, would record that the upper surface of the cloud in question closely resembled a snow field whose surface had been slightly ruffled into shallow parallel ridges. If the same imperfectly informed individual were judging simply from such appearances as are presented by the second photograph he would give a modified but scarcely more accurate description.

FIG. 1.

FIG. 2.

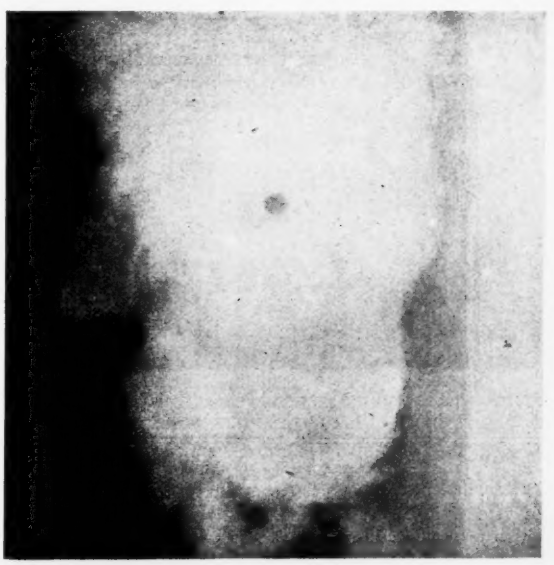
The true nature of the cloud limit as investigated from its own level will be discussed directly. Meanwhile it will scarcely need pointing out that what a balloonist commonly sees on a cloudy day at cloud level is simply a dense stratum of mist, such as constantly rests on the surface of the earth, often upwards of a thousand feet in depth, dark in its lower regions, but growing lighter as you ascend until, if the sun be brightly shining above, you are on reaching the fringe bathed in an atmosphere of the most dazzling luminosity. Then in a general way as you emerge you find yourself surrounded by huge fleecy billows tossed mountains high, the true regularity of which only becomes apparent when you have climbed high enough to look down on the billow crests and find them in arrangement (as shown in Fig. 1), somewhat resembling the surface of a storm-tossed sea, though they have always appeared to me to resemble more closely the arrangement of wreaths of smoke trailing behind a passing train, or streaming from an ill-stoked factory chimney, and due to the rhythmic "cling and release" of the gaseous cloud ere it issues from the aperture.

FIG 3.

A closely analogous appearance may often be noticed when a thin layer of snow on a level close-cut lawn has been thawing under the action of a breeze of some little strength. The snow may be seen to thin away, not so much in patches as in furrows, leaving broken but regular ridges lying athwart the direction in which the wind is blowing.

When cloudlets wander by at nearly the level of the observer—and it is astonishing how often such cloudlets though at close range will be found drifting at a different speed from that of the balloon—they are seldom of the roughly globular masses which, through foreshortening, they appear from below. They are generally rather elongated columns leaning forward with the wind. Larger detached cloud-masses may retain their heaped appearance, but as they are in a condition of wasting away their entire surface at a close view is frayed and withered.

Such a cloud is shown in the accompanying photograph, and bears the shadow of the balloon somewhat strikingly displayed upon it. Indeed this shadow, projected with



The Shadow of the Balloon may be seen upon the Cloud.

clearly-defined outlines upon so filmy a background, and surrounded, as it generally is, with iridescent rings, invisible in a photograph, but of great beauty, never fails to elicit expressions of delight and astonishment from those who behold it for the first time. The clouds I am here describing are in their nature and composition very different from the compact and newly-created cumulus cloud, whose surface is of closer texture, and whose level underside betrays so clearly the plane of demarcation of the air stratum above which it has been formed, and on the surface of which it, so to speak, floats. The most remarkable levelling of the under-surface of a cloud occurs perhaps when the cloud-masses are drifting up from a moist quarter, while below them is flowing a dry air stream, most often from the east. Under these circumstances the base of the cloud is, as it were, mown perfectly flat.

Another remarkable feature, to be noticed on the upper surface of clouds, and which has been revealed probably

XUM

only to the aeronaut sailing at their level, is when a cloud stratum assumes the appearance of long rollers, due apparently to the action of enormous waves of air. Such appearances have been more particularly noted on the Continent, where the rollers have been estimated as measuring a third of a mile from hollow to hollow, and in this strikingly bearing out Professor Helmholtz's theory with regard to the extent of atmospheric waves.

It is when a cloud stratum has been subjected for hours to the action of a hot sun that it will begin breaking up into chinks and hollows, which come as a revelation to the aerial voyager. These are seen by him when at a short distance above the cloud level long before any rifts in the cloud ceiling are noticeable from the earth. At first they appear as mere dark pits or clefts; and, if the sun be low, may scarcely be distinguished from the mere furrows or hollows of the cloud thrown into shade. But attentive watching will presently reveal the fact that brighter objects on the earth—roadways, water, or white buildings—are flitting past underneath.

Sometimes, however, it is the observer below whose vision can best gauge and penetrate a cloud. For instance, the density of an attenuated cloudlet is apt to be wrongly estimated by an observer who is immersed within it. Thus a balloon, as observed from the earth, may be only thinly veiled by a cloud which nevertheless completely blots out all objects from the aeronauts themselves.

It is no uncommon experience when level with a bed of stratus cloud to see cumulus cloud masses rising and rearing themselves into heaps above. A very similar phenomenon, and one of rare beauty, is often to be seen from aloft on overcast days, when perhaps a leaden sky overhangs all the land. Under such conditions I have found the cloud bed, which I have probably entered before an altitude of 2000 feet has been reached, to be upwards of 1000 feet thick, and the upper surface to be of one general level extending as far as the eye can reach. Here and there, however, this general level will be reared into a stately dome without assignable cause, and suggesting the thought that a mountain peak must be beneath. Professor McAdie has obtained some magnificent photographs from Mount Tamalpais of this appearance, which he has designated cloud pyramid, and which he has discovered to manifest itself over land which is perfectly level. The phenomenon may occur towards evening, when the upper air is beginning to reach a higher temperature, and the cloud column must be attributed to ascending currents. Instead of assuming the form of dome or pyramid the mass upreared will sometimes simulate the appearance of a vast wave breaking into spray, and attended as it were with veritable "spindrift" borne away on the gale. Under these circumstances, it seems little less than a mystery that such a fugitive and ethereal object can possess so much permanence as to remain practically unaltered in appearance for a long period.

There are certain days when though the sky may be devoid of actual cloud, it is full of an indefinite haze which is the ultimate form of cloud. On such days you may ascend, and by the time the first mile has been climbed you may be looking down on a universal lake of haze, the upper limit of which is as well defined and level as the surface of still water, while overhead the sky is absolutely clear.

As to the upper clouds which the balloonist in a general way does not reach, it can only be said that from ordinary "cloud level" they appear more clearly defined and nearer than when viewed from earth, not merely because they are actually beheld at closer range; but also because they are now seen in a transparent sky of purest and deepest blue.

MICROSCOPY

Conducted by M. I. CROSS.

THE MOUTH PARTS OF THE TSETSE FLY.

By W. WESCHÉ, F.R.M.S.

(*Continued from page* 92.)

There is little doubt that the suctorial mouth parts in the Mucidæ were evolved from an ancestor with complete biting mouth parts, and it is curious and interesting to find that this suctorial mouth is again modified in the Tsetse into a biting, or blood-sucking mouth. But there is no retrogression to the earlier type, the mandibles do not emerge from the upper part, or the maxillæ from the lower, but the development is on fresh lines. The lower lip has become horny, by a simple process of the chin plate, the *mentum*, having spread over its membrane, at the same time contracting it, and shrinking it into a slender tube. But at the extremity, instead of the tracheated discs, we find the tip modified into a series of laminated hard ridges, and at the extremity of these ridges the teeth, which in the suctorial mouth were inside and lower down (Figs. 1 and 2). The reason of these changes is obvious; it is necessary for the proboscis to become hard and strong in order to penetrate the thick skin of large herbivorous mammals. On examining the tip of the part with higher powers the resemblance of the laminated structure to that on the ovipositor of some flower-haunting Hemiptera

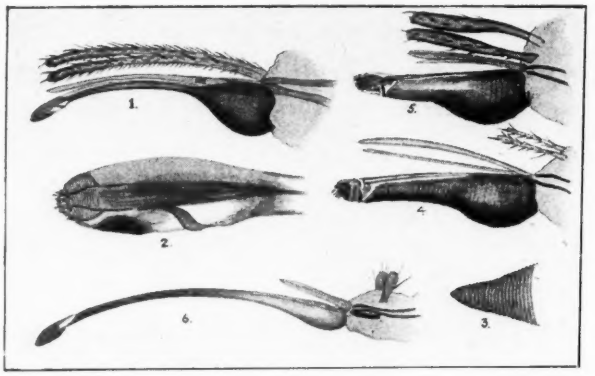


FIG. 1.—Mouth parts of Tsetse fly (*Glossina morsitans*). (All lateral views.) FIG. 2.—The tip of the proboscis of the Tsetse fly more magnified, showing chitinous ridges and teeth. FIG. 3.—Tip of ovipositor of *Capsus laniarus*, showing the chitinous ridges used in piercing stems of plants. FIG. 4.—Mouth parts of *Stomoxys calcitrans*, L., a very common British blood-sucking fly. FIG. 5.—Mouth parts of *Hæmatobia irritans*, L., a British blood-sucking fly. FIG. 6.—Mouth parts of *Prosena sybarita*, F., a British flower-feeding fly.

is evident. The ovipositor in these insects is used to bore into wood (Fig. 3). On the suctorial mouth there are two transverse levers which expand the discs; these are very well marked, and obviously homologous in the Tsetse fly. The lancet (hypopharynx) seems to have weakened and the upper lip to have atrophied. This is probably occasioned by disuse, though in *Stomoxys* both these organs are present (Fig. 4). The palpi have greatly lengthened, showing that they have important work to do. In our English *Hæmatobia irritans* this has also taken place, but in *H. stimulans* and *Stomoxys calcitrans* we find them of the usual size, or even in a degenerate state (Fig. 5).

The proboscis of *Prosena sybarita* is even more like that of the Tsetse than those mentioned, but its use is quite different; it is used to suck the juices from the long tubes of flowers, but

there can be little doubt that the development has been on similar lines, though *P. sybarita* has the trachæ still showing at the end. This is a stage through which *Glossina* undoubtedly passed before it developed the chitinous ridges that now characterise it (Fig. 6). The large swelling at the base of the proboscis was for some time thought to be the receptacle for the Tsetse poison. This was plausible, but has no foundation in fact, as this part is simply full of the complicated muscles necessary for the working of the proboscis. The herptomonads which are the pathological element are an accident, and even an unfortunate accident regarded from the Tsetse point of view. They form part of the food of the insect, sucked with the blood from the antelopes and zebras, and may be found in any part of the alimentary canal. Till the Tsetse has sucked the blood of an infected animal it is like the Anopheles mosquito in a similar state, perfectly harmless.

I have been asked to say where specimens of Tsetse can be obtained. I am afraid that they are very difficult to come by. The only chance is through friends in Africa, and that chance but a remote one, as man avoids the Tsetse country as much as possible. Even if the flies can be caught it requires a certain measure of skill to set them properly, and they will probably arrive in a very unsatisfactory state.

In the Natural History Museum at Cromwell Road is one of the earliest specimens obtained. This was sent home by Dr. Livingstone, and is simply the insect crushed on a piece of paper. This is cherished as a relic of the great explorer, but is a sorrowful object to an entomologist. I have also seen some that another great traveller, Captain Speke, collected; these were properly pinned and set, and, considering how many years they have been in the Museum, are in fairly good condition.

The figures are drawn from preparations, cleared and mounted with pressure.

"THE HELMHOLTZ THEORY OF THE MICROSCOPE."

The meeting of the Royal Microscopical Society on March 18th was devoted to a paper by Mr. J. W. Gordon on "The Helmholtz Theory of the Microscope." Mr. Gordon's paper was suggested by an article which Helmholtz contributed in 1874 to *Poggendorf's Annalen* under the title, "The Theoretical Limit of Resolving Power in the Microscope"; "the merit of which"—in Mr. Gordon's opinion—"lies not in this formal result (that is, the demonstration of a definite limit of resolving power), which is, in fact, not successfully established, but in the line of investigation which Helmholtz here strikes out, and without following it to its practical issues, pursues far enough to present his readers with a surfeit of interesting and valuable suggestions. These suggestions are, many of them, so obscure and conveyed by such subtle hints that they may well escape attention, and apparently they have escaped attention to the present day."

Mr. Gordon attempted the extremely difficult task of giving an account of Helmholtz's paper without "its somewhat repulsive mathematical garb," a task the more difficult in the present case because Helmholtz himself, in a postscript, frankly acknowledging the priority of Prof. Abbe with regard to the enunciation of the several theorems contained in his paper, suggests as the latter's only scientific justification just the mathematical proofs of those theorems which Abbe had not then published.

Briefly stated, Mr. Gordon's paper gives first a non-mathematical dissertation on the theory of diffraction and diffraction gratings, which, he says, Helmholtz takes for granted, and writes as abstrusely about as the most hardened mathematician. Then follows a long section on the famous Sine-condition, which has probably suffered most—as compared with Helmholtz's lucid paper—by the effort to suppress mathematics. Finally the contraction of the emergent pencils with increasing magnifying power is discussed as affecting the brightness of the image, the obtrusiveness of entoptic appearances, and especially the rapid widening of diffraction fringes.

It is in this last section that Mr. Gordon finds fault with Helmholtz's reasoning and endeavours to prove him in the wrong. It is, however, to be feared that not many scientifically educated microscopists will prefer Mr. Gordon's arguments to those of the great physicist.

Having convinced himself that Helmholtz's reasoning as to the limit of resolution is quite wrong, Mr. Gordon once more tries to suggest improvements of the microscope which would or might extend its powers. It will be remembered that about two years ago, in a paper read before the Royal Microscopical Society, Mr. Gordon suggested that the "antipoints" might be made as small as desired, and the definition accordingly improved, by some arrangement of lenses behind the objective. This time we are offered an oscillating screen. The image projected by the objective is to be focussed on a finely ground glass-screen, which is, moreover, to be set oscillating in order to render its grain invisible; and the image so projected is to be viewed by a second microscope so as to get a high magnification without an excessively contracted emerging pencil.

In a first appendix, Mr. Gordon gives what he believes to be a proof of the Sine-law, or, rather, of a Sine-tangent law; he also endeavours to show that Hockin's elegant proof—as accepted by Prof. Sir Silvanus Thompson, Dr. Czapski, and other authorities—is wrong. In a second appendix he tries to prove by a kind of graphical integration that lines much closer than would follow from Helmholtz's formula can be resolved.

A third note tries to prove Helmholtz in the wrong as regards the impossibility of suppressing diffraction-phenomena; and a fourth points out an error in a published translation of Helmholtz's paper.

The late hour at which the reading of the paper was concluded unfortunately cut short what might evidently have been an interesting and instructive discussion. Still, it sufficed to show a strong conviction in certain quarters that Mr. Gordon had over-estimated the importance of "the Helmholtz theory," and had, at the same time, not done justice to the lucid reasoning of the original paper.

Mr. Gordon's interpretation of the Sine-law was particularly objected to (he maintained that it applied to objects and images of sensible size, whereas it is obviously, as a strict mathematical theorem, limited to a surface element in the optical axis), and it was pointed out that he had not given Helmholtz's proof of the Sine-law proper at all, which Helmholtz deduced by integration from a more general one applying to all centred optical systems, but restricted to small angles of divergence and of incidence. Considerable doubt was also expressed whether the oscillating screen would put any detail into the image which could not be seen with a suitable eyepiece.

A careful perusal of the paper and a comparison with the original German paper of which it is meant to give an account must convince anyone familiar with optical and microscopical theory that it bristles with statements which invite criticism.

MONOCHROMATIC LIGHT (*Continued*).

The following list of light filters, any of which can be easily made, and will be found invaluable in photo-micrography especially, have been tried in actual working. They are from the formulæ of Dr. Nagel, of Freiburg, and with others of a similar nature are detailed in "Biological Laboratory Methods," by Mell.

ORANGE FILTER for the spectrum district between C and D.—Prepare a solution of acetate of copper, add a few drops of acetic acid, and then, drop by drop, a concentrated saffranin solution, until the solution will admit no more violet, blue, green and yellow light.

YELLOW-RAY FILTER.—Add to a saturated acid solution of acetate of copper a saturated acidified solution "Orange G." The liquid has a brown appearance, and passes only a small stripe of yellow light.

GREEN-YELLOW FILTER.—To a saturated solution of bichromate of potassium acidified with acetic acid, add crystals of acetate of copper, and heat the solution. The green liquid passes only monochrome green light.

BLUE-RAY FILTER.—A weak solution of methyl-green is mixed with acetate of copper solution until no red light passes.

Reference was made in the last number to the Gifford Screen for line F, and the special qualities of the acetate of copper screen, which has been previously described in these columns, should not be overlooked. This screen consists of a saturated solution of acetate of copper. The solution is placed in a trough which should have an interval between its back and front glasses of at least $\frac{5}{8}$-inch. $\frac{5}{8}$-inch exactly absorbs the red end of the spectrum when an oil lamp having a $\frac{1}{2}$-inch wick is used. If a more brilliant illuminant be employed an increased width of trough might be also used without disadvantage.

XUM

NOTES AND QUERIES.

W. N. T.—The merits of all the microscopes you name are so close that it is impossible to recommend one in preference to another. So much depends on your future intentions, and if you propose to do progressive work preference should be given to an instrument which can have extra fittings added to it from time to time to make it quite complete. For the work you name, you would find a No. 2, or "B," ocular, in conjunction with 1 inch and $\frac{1}{2}$ inch objectives, the most serviceable. It is unlikely that you would require both the $\frac{1}{4}$ inch and $\frac{1}{6}$ inch; and I certainly should not advise the use of a deep-power eyepiece for the comparatively low-power objectives for continual work. Better results are always secured with eyepieces of low power.

W. Rogers.—(1) *Treatment of Fossil Deposits for Diatoms.*—Break deposit up into small pieces and boil in a strong solution of soda bi-carbonate, pour off from time to time the disintegrated material into a beaker, and continue the boiling until all the deposit has broken up. The soda solution must then be washed away and the diatoms boiled for about 15 minutes in nitric acid, and, when clean enough, wash away the acid with repeated changes of water, then boil up the diatoms in distilled water. (2) *Treatment of Sponges.*—If horny, boil in liq. potassæ, then wash spicules with water to remove all trace of potash and mount them in glycerine jelly or Canada balsam. Siliceous sponges must be boiled in nitric acid, then washed well with water and mounted in glycerine jelly or Canada balsam. For types in which siliceous spicules are embedded in horny matrix, boil in liq. potassæ for a few minutes to disintegrate the spicules, then boil in nitric acid to clean spicules, wash away acid with repeated changes of water and mount in glycerine jelly or Canada balsam. (3) *To make Camphor Water.*—Add some pieces of camphor to distilled water and let it stand for 24 hours, filter, and then you will have a 3 per cent. solution; water will not take up more. Another way: Dissolve some camphor in rectified spirit, add a few drops of distilled water, shake well for a minute, filter, and keep in a stoppered bottle. (4) I consider a 2 inch objective better for small seeds.

C. Zimmermann.—(1) The parasite on the portion of pear tree you send is the Pearl Oyster Scale (*Aspidiotus ostreæformis*). (2) The only fixing medium I know of for celloidin sections is Mayer's albumen, but I do not think any is required; the celloidin need not be removed. The tissue should be stained in bulk, embedded in celloidin, cut sections, place in absolute alcohol for about three minutes, clear in oil of origanum, and mount in Canada balsam. The celloidin will be invisible. (3) There is only one solvent for celloidin, viz., equal parts of absolute alcohol and ether.

Communications and enquiries on Microscopical matters are cordially invited, and should be addressed to M. I. CROSS, KNOWLEDGE *Office, 326, High Holborn, W.C.*

NOTES ON COMETS AND METEORS.

By W. F. DENNING, F.R.A.S.

GIACOBINI'S COMET (1902 d).—This small, distant comet is still visible in the evenings, and will be situated about 2° E.N.E. of α Geminorum at the opening of May, and moving slowly eastwards. Recent observations have fully confirmed the exceptionally great perihelion distance (258 millions of miles) derived from earlier computations. This distance is 118 millions of miles outside the mean position of the orbit of Mars and in the region of the minor planets. On May 1 the comet will be about 280 millions of miles from the earth.

PERIODICAL COMETS.—Several short-period comets, belonging to the Jovian family, are due to return to perihelion this year, but the conditions will be unfavourable in most cases so that the objects will probably escape observation. Giacobini's comet of 1896 (V.), formerly supposed to have a period of 9 years, appears to revolve in only 6 647 years according to new elements computed by Ebell (*Ast. Nach.* 3848). It will return during the present spring, but only as a very faint object. Perrine's comet of 1896 (VII.) should pass through perihelion on April 26, and Ristenpart gives elements corrected for perturbation by Jupiter in *Ast. Nach.* 3841. But the prospect of seeing the object is extremely small, for when at its brightest in May it will have only half the light which it displayed when last seen in 1897 at Northfield, Minn., on which occasion the aspect of the comet was described as "most exceedingly faint" as viewed in a 16-inch refractor. Spitaler's comet, 1890 (VII.), is due in the summer, but it is a feeble object, and will not be sufficiently well placed to be re-observed. Faye's well-known comet (1889 (V.)—1896 (VI.)) will reach perihelion next December, but will be nearest to the earth at the middle of August. Its periodic time is 2592 days (7·1 years), and Seagrave has computed a search ephemeris from which we hope to give extracts in later months.

SOUNDS ACCOMPANYING METEORS AND METEORITES.—Casual observers sometimes report that they distinctly heard, simultaneously with the flight of a meteor, a hissing, rushing or whirring sound. Many instances of this might be quoted from descriptions of meteoric apparitions in recent years. But judging from the circumstances and from the nature of the evidence there seems little reason to doubt that in all such cases the contemporary sounds were either imaginary or produced from sources altogether different to those assigned. Sound is a slow traveller, and even admitting that a meteor penetrates the air to within 25 miles of an observer, the noise of its disruption or concussion could not reach him until 2 minutes afterwards unless there is some law of acoustics not yet understood.

In the case of a meteorite close to the earth's crust, and in the act of subsiding upon it, the conditions are essentially dissimilar, for its detonations may startle an observer before he sees the falling body strike the earth. This arises from the remarkable slowness of motion with which the object is travelling. Meteorites usually penetrate the soil to the depth of only a foot or so, from whence it is evident that their velocity can scarcely exceed a few hundred feet per second. The meteorite which fell at Wold Cottage, Yorks, on 1795, December 13, was dug out of a hole 18 inches deep. The meteorites of 1876, April 20, and 1902, September 13, embedded themselves to the same depth as that of 1795, while that of 1881, March 14, was discovered 11 inches below the surface.

The question of the precedence of sound in attracting attention to these events formed the subject of an interesting discussion at a recent meeting of the British Astronomical Association, but the matter seemed difficult to explain, as the speakers appeared to regard parabolic or planetary speed as one of the necessary features of falling meteorites. But atmospheric resistance, and, in lesser degree, terrestrial attraction, must exercise a great obstructive influence upon objects of this class, robbing them of their initial speed and direction, and ultimately causing them to descend upon the earth with a velocity comparable with that of terrestrial bodies. When the meteorite of 1881, March 14, reached the earth's surface its velocity was only 412 feet per second (1 mile in 13 seconds) according to some experiments specially carried out by Prof. Herschel. The object would have acquired this rate by falling freely through $\frac{1}{2}$ mile, while if vertically descending through 40 miles it would have reached the ground with a velocity nine times as great as that observed. "Schiaparelli has shown that if the law of resistance for planetary motions is similar to that derived from experiments with artillery, then a ball of 8 inches diameter and 32$\frac{1}{2}$ lbs. weight entering the atmosphere with a velocity of 44$\frac{3}{4}$ miles per second will on arriving at a point where the barometric pressure is still only $\frac{1}{700}$ of that at the earth's surface, have its velocity already reduced to 3$\frac{1}{3}$ miles a second." Such meteors, therefore, as are sufficiently large and compact to be practically stopped in their courses before disruption and dissolution, will become cooler, and descend leisurely to the earth, sometimes travelling with a velocity less than that of sound, so that the intimation of their downfall is conveyed to the ear quicker than to the eye of a spectator who may happen to be near.

THE FACE OF THE SKY FOR MAY.

By W. SHACKLETON, F.R.A.S.

THE SUN.—On the 1st the sun rises at 4.37 and sets at 7.19; on the 31st he rises at 3.53 and sets at 8.2.

The minimum of sunspot activity seems now to be left well behind, and spots of considerable size have been recently recorded. At the time of writing there is a fairly large spot near the central meridian.

THE MOON:—

	Phases.	H. M.
May 4	☽ First Quarter	7 26 A.M.
„ 11	○ Full Moon	1 18 P.M.
„ 19	☾ Last Quarter	3 18 P.M.
„ 26	● New Moon	10 50 P.M.

The moon is in perigee on the 1st and 28th, and in apogee on the 16th.

The following are the two principal occultations visible at Greenwich during the month:—

Date.	Star Name.	Magnitude.	Disappearance. Mean Time.	Disappearance. Angle from N. Point.	Disappearance. Angle from Vertex.	Reappearance. Mean Time.	Reappearance. Angle from N. Point.	Reappearance. Angle from Vertex.	Moon's Age.
			h. m.	°	°	h. m.	°	°	d. h.
May 2	68 Geminorum	5·0	7 25 P.M.	86	54	8 27 P.M.	301	263	5 6
„ 12	χ Ophiuchi	5·0	8 7 P.M.	129	166	9 5 P.M.	257	290	15 7

There are several "near approaches" during the month, notably that to λ Geminorum on the 29th at 9.5 P.M., the star just escaping occultation.

THE PLANETS.—Mercury is well placed for observation during the evenings of the early part of the month. The *greatest elongation* of 21° 31′ E takes place on the 10th, when at sunset the planet is fairly high up in Taurus, being then 2¼ hours above the horizon. Near the time of greatest elongation the planet precedes Venus by about an hour, so that with the help of the demarcation of the ecliptic by the line joining Venus and the sun it should not be difficult to locate the position of Mercury.

Venus is the most prominent object in the western sky after sunset, and cannot fail to attract the attention by her brilliancy. On the 1st she sets at 10.46 P.M., and on the 31st at 11.19 P.M. The apparent diameter of the planet is increasing, being 15″·0 at the middle of the month; also the phase is becoming prominently gibbous, 0·74 of the disc being illuminated. On the evenings of the 20th and 21st the planet will be close to the star ε Geminorum, and on the evening of the 29th she will be in proximity to the moon.

Mars is an evening star in Virgo, and is available for observation throughout the month from sunset until after midnight, setting on the 1st at 3.41 A.M., and on the 31st at 1.39 A.M. There is no mistaking the planet, since he is the brightest object in the sky looking southwards, and also on account of his ruddy colour. The bright star Arcturus has a somewhat similar appearance, but it is not so bright, and further has a greater altitude. With small telescopes one sees the disc and that there are markings, but it is difficult to delineate them; also the polar snow cap is not showing to advantage. About the middle of the month the diameter of the planet is 12″·0, whilst 0·926 of the disc is illuminated. He is near the moon on the evening of the 7th, and ends his retrograde motion in Virgo on the 11th.

Jupiter and Saturn both rise after midnight. Near the middle of the month the former rises about 2 A.M., and the latter about 1 A.M.

Uranus is in Ophiuchus, rising on the 1st at 11.13 P.M., and on the 31st at 9.10 P.M. On account of his extreme southerly declination, however, he is not favourable for observation this month at convenient times.

Neptune is only available for observation during the earlier parts of the month. On the 19th he is very close to η Geminorum, having the same R.A. as the star, and being only 0° 9′ to the south; in fact during the whole month he will appear in the same field of view as the star if not too high a power be used. A few observations, therefore, will detect him on account of his changing position.

THE STARS.—About 10 P.M. at the middle of the month Ursa Major will be nearly overhead; Arcturus a little east of south, and Spica Virginis on the meridian; Leo in the south-west, and Gemini in the north-west; Cygnus in the north-east; Vega high up in the east; Scorpio rising in the south-east.

Chess Column.

By C. D. LOCOCK, B.A.

Communications for this column should be addressed to C. D. LOCOCK, Netherfield, Camberley, and be posted by the 10th of each month.

Solutions of April Problems—(By C. D. Locock).

No. 1.

1. R to R5, and mates next move.

No. 2.

1. Q to Kt4, and mates next move.

SOLUTIONS received from "Alpha," 2, 0; W. Nash, 2, 2; G. A. Forde (Major), 2, 2; "Looker-on," 2, 2; A. H. H. (Croydon), 2, 2; W. H. S. M., 2, 2; G. W. Middleton, 2, 2; "Tamen," 2, 0; "Quidam," 2, 2; J. W. Dixon, 2, 2; H. F. Culmer, 2, 2; T. Dale, 2, 2; A. H. Doubleday, 2, 2; W. J. Lawson, 0, 2; C. Johnston, 2, 2.

G. Heathcote and H. F. W. Lane.—Many thanks for your letters.

J. W. Dixon.—Glad to hear that you appreciate the prize problems.

A. Lillie, J. C. Candy, P. G. L. F., H. N. Fellows.—Thanks for the problems, which I shall hope to publish in the course of the year.

Hamilton White.—I am afraid that I cannot be drawn by your counter-proposal that I should send you the solution of your own end-game. In stating that Black's moves were not all forced, I erroneously put "Black" for "White," Black being the usual defending side in problems and end-games for publication. You will see, of course, that *White's* 4th, 5th and 6th moves are not forced. The promotion to a Knight may be a little unusual in actual play, but I consider, nevertheless, that the whole *modus operandi* is a little too obvious in spite of the testimony of well-known players in its favour.

G. A. Forde (Major).—Yes, the solution of No. 2 last month was inaccurately given.

Alpha.—I discarded No. 2 many years ago on account of the second solution by 1. Q to Q7; but the Black Pawn at Kt2 now prevents this.

W. J. Lawson.—If 1. B to Q3, P × Kt.

"*Tamen.*"—Please see reply to "Alpha" above.

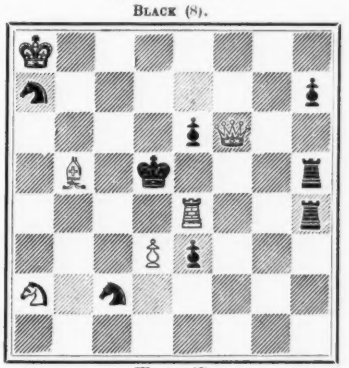
W. Jay.—I much regret that your solutions this month were too late. The issuing post-mark is "1 P.M., April 11."

PROBLEMS.

No. 1.

By W. Geary.

BLACK (8).



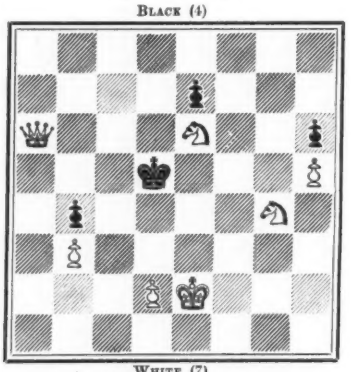
WHITE (6).

White mates in two moves.

No. 2.

By T. Geary.

BLACK (4)



WHITE (7)

White mates in three moves.

CHESS INTELLIGENCE.

The Inter-University Match took place on March 23rd, the scene of play, after the lapse of many years, being once more the St. George's Chess Club. The result was a rather decisive victory for Cambridge. Mr. Pillsbury officiated as adjudicator, and the score was:—

CAMBRIDGE.		OXFORD.	
H. A. Webb, Trinity	1	H. F. Davidson, Exeter	0
H. Bateman, Trinity	1	H. D. Roome, Merton	0
B. G. Brown, Trinity	½	T. H. Bumpas, St. John's	½
G. Leatham, St. John's	0	W. M. Grundy, All Souls'	1
D. Glavert, Clare	1	H. Taylor, Balliol	0
L. H. Goh, Emmanuel	1	S. C. von Ernsthausen, Balliol	0
T. Lodge, Trinity	1	S. N. Foster, Worcester	0
	5½		1½

On March 27th and 28th, Oxford and Cambridge combined forces to play their fifth match by cable against the American Universities. The result depended on Mr. Pillsbury's adjudication on the game at the last board, and the award of this game to the English team gave them the victory. Score:—

OXFORD & CAMBRIDGE.		AMERICAN UNIVERSITIES.	
H. A. Webb (Camb.)	½	C. T. Rice (Harvard)	½
H. Bateman (Camb.)	0	F. H. Sewell (Columbia)	1
H. F. Davidson (Oxf.)	1	J. F. Sawin (Yale)	0
H. D. Roome (Oxf.)	0	— Richardson (Princetown)	1
T. H. Bumpas (Oxf.)	1	H. A. Keeler (Columbia)	0
B. Goulding-Brown (Camb.)	1	— Bridgeman (Harvard)	0
	3½		2½

The Anglo-American cable match for the Newnes Challenge Cup was played on April 3rd and 4th. Messrs. Pillsbury and Marshall, being in London at the time, played their games over the board, as last year, and against the same opponents. The result in Mr. Pillsbury's case was again a drawn game against Mr. T. F. Lawrence, the City of London champion, but Mr. Marshall took his revenge on Mr. Atkins. Mr. Blackburne played in his most ingenious style against Mr. Barry, but eventually succumbed under great pressure of the time limit. Mr. Mills drew with Mr. Hodges, as everyone predicted. It does not speak well for the improvement of English chess, that the two players new to the team were players of such long standing as Messrs. Gunston and Hooke. The former of these, a very sound player, and an expert in correspondence games, certainly justified his inclusion. The Americans retained possession of the trophy; should they do the same next year it will become theirs finally. The score was as under:—

ENGLAND.		AMERICA.	
T. F. Lawrence	½	H. N. Pillsbury	½
J. H. Blackburne	0	J. F. Barry	1
D. Y. Mills	½	A. B. Hodges	½
H. E. Atkins	0	F. J. Marshall	1
G. E. Bellingham	1	E. Hymes	0
H. W. Trenchard	0	H. Voight	1
R. P. Michell	1	C. J. Newman	0
Herbert Jacobs	½	E. Delmar	½
W. H. Gunston	1	C. Howell	0
G. A. Hooke	0	H. Helms	1
	4½		5½

At Monte Carlo, last month, Dr. Tarrasch, of Nuremburg, won his sixth international tournament, and this in spite of the fact that he scored only 2½ out of his first six games. Nowadays, owing to want of practice, he is always a bad starter, but he gradually recovers all his old form, and in a long contest like that under notice he is sure to be near the top. For a long time Messrs. Pillsbury and Teichmann held the lead, but ultimately lost their places, and were passed by Maroczy. In the absence of Lasker, Janowski, and Tchigorin, the tournament was, of course, not quite representative of international strength, but there seems to have been only one really weak player. The final score was:—First Prize, Dr. S. Tarrasch, 20; Second Prize, G. Maroczy, 19; Third Prize, H. N. Pillsbury, 18½; C. Schlechter, 17; R. Teichmann, 16½; G. Marco, 15½; F. Wolf, 14; J. Mieses, 13; F. J. Marshall, 12; J. Mason and S. Taubenhaus, 10½; A. Albin, 8; Reggio, 7½; Col. Moreau, 0.

Yet another international tourney begins at Vienna on the 1st of this month, the "King's Gambit accepted" being compulsory in every game.

Brighton Society announces a three-move problem tournament in connection with *Das Neue Illustrirte Blatt*, the idea being to ascertain what kind of problem will best fall in with the widely differing views of English and Continental judges. Mr. B. G. Laws will be the English judge. Entries from Great Britain should reach Mr. Max J. Meyer (Chess-Editor of *Brighton Society*), St. Charles, Percy Road, Bournemouth, not later than May 31st.

Mr. H. N. Fellows, of 74, Curzon Street, Wolverhampton, has issued a short and clearly-written pamphlet, reprinted from the *Wolverhampton Journal*, on the art of solving three-move chess problems. The price of the leaflet, which is intended for beginners, is 2d. post free.

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